Political Economy in a globalized world

How politics, culture, and institutional incentives shape economic and political outcomes

DISSERTATION

April 14, 2015

Verfasst von: Kai Gehring

Dissertation:

zur Erlangung des wirtschaftswissenschaftlichen Doktorgrades der
Wirtschaftswissenschaftlichen Fakultät der Universität Göttingen

Vorgelegt von:

Kai Gehring

Geburtsort: Karlsruhe
Geburtsdatum: 29.04.1985

Erstgutachter: Prof. Dr. Axel Dreher

Zweitgutachter: Prof. Stephan Klasen, Ph.D.

Drittprüfer: Prof. Dr. Thushyanthan Baskaran

Tag der mündlichen Prüfung: 22. Mai 2015

Danksagung

Ist es wirklich schon vorbei? Tatsächlich kommt es mir so vor, als wären die vergangenen dreieinhalb Jahre wie im Fluge vergangen. Ich hatte das Glück, einen Doktorvater zu haben, der mich sehr früh ermuntert und es mir ermöglicht hat, meine Arbeiten wissenschaftlich begutachten, auf Konferenzen vorzustellen und veröffentlichen zu lassen. Keinerlei „Welpenschutz" also am Lehrstuhl Dreher, dafür die Möglichkeit, sich früh zu beweisen und als vollwertiger Wissenschaftler zu fühlen.

Ich kann mich erinnern, wie ich stolz nach dem Abschluss meiner Diplomarbeit dachte, wenn ich das jetzt in fünf Monaten geschafft habe, dann sollte es ja nicht zu schwer sein, in 3-4 Jahren Promotion genügend Studien für einen erfolgreichen Abschluss zu verfassen. Tatsächlich war ich in der glücklichen Lage, aus meiner Diplomarbeit meinen ersten wissenschaftlichen Beitrag verfassen zu können, der auch noch relativ schnell veröffentlicht wurde (Gehring 2013). Trotzdem wurde nach kurzer Zeit in der Wissenschaft klar, dass die Anforderungen hier in Bezug auf Originalität, Anspruch und Komplexität doch ein ganz anderer als innerhalb des Studiums ist. Wie viele dutzend Ideen letztlich doch im Papierkorb gelandet sind, obwohl sie sich anfangs so vielversprechend angehört hatten. Ja, wie viele Projekte ich tatsächlich aktiv begonnen habe, um dann nach einigen Wochen oder teils Monaten zu realisieren: Nein, das ist reicht nicht aus für meinen Anspruch, und/oder für den Anspruch meines Doktorvaters.

Der eigene Anspruch ist ein interessanter Aspekt. Dieser steigt tatsächlich (leider) mit der Dauer der Promotion immer weiter an. Dinge, die mir am Anfang wohl noch innovativ und zuverlässig erschienen sind, wirkten später trivial oder nicht mehr ausreichend. Eine der Hauptherausforderungen meiner Promotion war, zwischendurch zu realisieren, was ich eigentlich schon alles erreicht habe, und nicht direkt ohne Unterlass auf das nächste Ziel zuzusteuern. Einen Moment innezuhalten, das Erreichte Revue passieren zu lassen, und einfach mal in Ruhe über neue Ideen und Projekte nachdenken war tatsächlich schwieriger als ich mir anfangs gedacht hatte. Der Alltag, zumindest meiner, während der Promotion war doch wesentlich „voller" und kompakter, als ich mir das vielleicht manchmal erhofft hätte.

Im Nachhinein erscheint es mir immer noch unglaublich, was ich alles während dieser Zeit erreichen konnte und erleben durfte. Neben meinen ersten eigenen Übungsveranstaltungen in Heidelberg begann ich auch als externer Dozent eine eigene Veranstaltung an der Fachhochschule Kaiserslautern zu leiten. Die Freiheit hierbei, tatsächlich meine eigene Veranstaltung mit selbstbestimmtem Curriculum zu erstellen, und das Vertrauen der dortigen Verantwortlichen haben mich sehr gefreut. Generell hat mir das Lehren retrospektiv immer sehr viel Spaß und Freude bereitet. Im Gegensatz zur von Zeit zu Zeit langwierigen Arbeit als Wissenschaftler bekommt man hier ein direktes Feedback der

Studierenden, und kann so auch mal ein kurzfristiges Erfolgserlebnis verbuchen. Ich hoffe, diese Begeisterung mit in meine nächste Stelle zu nehmen, wo ich dann zwei eigene Vorlesungen gestalten und unterrichten werde.

In Bezug auf die Forschung freut es mich vor allem, wie viele Projekte ich trotz Vielzahl abgebrochener Projekte und verworfener Ideen während dieser Jahre abschließen konnte. Dahinter stecken natürlich auch eine Vielzahl durchgearbeiteter Wochenenden, und nicht genommener Urlaubstage, aber es ist schön zu sehen, dass sich harte Arbeit auch auszahlt. Da Rückschläge unweigerlich zur Karriere als Wissenschaftler gehören, freue ich mich umso mehr, mit dem Abschluss der Promotion einen Meilenstein zu erreichen.

Und trotz aller Arbeit und Anstrengung kamen auch der Spaß und das Abenteuer nicht zu kurz. Ich hatte das Glück, auf dieser „Reise" eine große Zahl toller Kollegen zu haben, von denen ich viele inzwischen auch als gute Freunde zu schätzen weiß. Angefangen hat alles 2011 mit den damaligen Kollegen in Heidelberg, die mich sofort als Teil des Teams willkommen geheißen haben. Sehr gern erinnere ich mich an unsere Anfänge als wahrhaft multikulturelles Team zurück, an viele interessante Lehrstuhlabende, gemeinsame Mittagessen mit angeregten Diskussionen und generell einfach eine gute und angenehme Atmosphäre am Lehrstuhl. Das Team hat sich natürlich verändert, aber glücklicherweise beweist Axel immer ein glückliches Händchen bei der Personalauswahl, und wir sind und waren eine tolle Gruppe, in der einer dem anderen hilft und mit Rat und Tat zur Seite steht.

Das gleiche gilt uneingeschränkt für all meine Kollegen und Freunde in Göttingen und Hannover, die mit mir zusammen die erste Kohorte des RTG 1723 gebildet haben. Schon bei unserem ersten Treffen beim Kennenlernwochenende in Goslar war klar, dass wir eine bunte, offene und natürlich talentierte Gruppe darstellen. Während des ersten Jahres verbrachte ich noch viel Zeit in Göttingen und Hannover, was dann mit zunehmender Arbeitsbelastung durch Forschung und Lehre, und dem Auslaufen des Kursprogramms abnahm. Gerne hätte ich noch mehr Zeit in Göttingen verbracht, aber anstatt an dieser Stelle wehmütig zu werden, freue ich mich über die schönen Momente, die ich dort hatte.

Auch in Bezug auf Konferenzreisen als elementarer Bestandteil des wissenschaftlichen Arbeitens hatte ich das Glück, durch meinen früh verfassten ersten Artikel bald aktiv teilnehmen zu können. Eine meiner positivsten Erfahrungen und Eindrücke als Wissenschaftler waren die Offenheit etablierter Forscher auf Konferenzen und bei Seminaren. Von Cambridge bis Oxford, von Harvard bis Princeton, Luzern bis Zürich, von einem Vortrag bei den Vereinten Nationen in Kopenhagen bis zum Finanzministerium in Berlin durfte ich an vielen tollen und spannenden Veranstaltungen teilnehmen, Erfahrung sammeln, und interessante Leute kennenlernen.

Insgesamt hatte meine Promotion so viele unterschiedliche Stadien, die jeweils mit spezifischen Orten und Menschen verbunden sind. Anfangs im Herbst 2011 die

Doktorandenkurse in Mannheim; sozusagen eine (weitere) harte Prüfung bis ich endlich mit eigener Forschung beginnen konnte. Dann das weitere Jahr mit intensivem Kursprogram in Göttingen und Hannover, welches ich durch eine Bahncard100, viel Pendeln und nette Freunde in Göttingen und Hannover (die mir immer wieder einen Platz auf ihrem Sofa oder Gästebett offerierten) ziemlich problemlos und mit großem Erkenntnisgewinn absolviert habe. Danach „endlich" das selbstständige Arbeiten als Wissenschaftler, jetzt häufiger von Heidelberg aus. Im Jahr 2014 bekam ich dann noch die unvergessliche Möglichkeit, ein Semester als Gastforscher an der Harvard University zu verbringen. Hier habe ich wirklich nochmal viel gelernt, weitere faszinierende Forscher kennengelernt und mich intellektuell inspirieren lassen.

Ich bin einer Vielzahl an Menschen zu tiefer Dankbarkeit verpflichtet! In Bezug auf meine Kollegen möchte ich die meisten hier nicht namentlich nennen, auch damit sich keiner benachteiligt fühlt. Ich habe mich immer glücklich geschätzt, so tolle Kollegen und Freunde zu haben, und gerade der individuelle Austausch macht einerseits die Wissenschaft aus und hilft andererseits, mit Rückschlägen umzugehen. Namentlich nennen möchte ich Vera Eichenauer, mit der ich jetzt seit Jahren das Büro teile. Es war immer sehr hilfreich, sich über wissenschaftliche Probleme austauschen zu können, aber ebenso wichtig sich von diesen abzulenken, sei es mit einer Runde Yoga in unserem Miniaturbüro oder einer kurzen und intensiven Diskussion über Gott und die Welt. Auch Andreas Fuchs möchte ich noch erwähnen, mit dem ich einerseits über mehrere Jahre an einem wichtigen Teil der kumulativen Dissertation gearbeitet habe, und von dem ich auch viel über ordentliches, strukturiertes Vorgehen und die Bedeutung harter Arbeit und Sorgfalt gelernt habe. Auch wenn es anstrengend war, werde ich mich immer gerne an unsere stundenlangen Diskussionen, teils bis tief in die Nacht hinein, erinnern. Mit dieser Liebe zum Detail und Streben nach Perfektion sollte Wissenschaft betrieben werden.

Einen Dank auch an eine Vielzahl wissenschaftlicher Hilfskräfte, welche mich über die Jahre auf vielfältige Art und Weise unterstützt haben. Mir war es immer wichtig, bei diesen Arbeiten auch Sinn und Zweck einer Aufgabe zu erläutern, um auch einen Lern- und Erkenntnisfortschritt zu fördern. Bei manchen sehr langwierigen und monotonen Aufgaben ist das, wie ich aus eigener Erfahrung weiß, nicht immer einfach. Besonders erwähnen möhte ich dabei unsere Fremdsprachenlektoren; ihre Arbeit erleichtert das Verständnis der englischen Texte sicherlich noch einmal beträchtlich. Danke für die gute Arbeit über die Jahre hinweg.

Privat ist so eine Promotion natürlich auch nicht immer einfach, schon allein aufgrund der Arbeitsbelastung. Danke an Angelika Spohn, die mich trotz mancher Probleme einen großen Teil dieser Zeit begleitet und unterstützt hat. Generell freue ich mich, dass ich auch in schwierigeren Zeiten Freunde, Verwandte und meine Eltern hatte auf die immer Verlass war. Danke für eure Denkanstöße, eure Unterstützung und auch immer wieder für die nötige Ablenkung von der immer doch zumindest im Hinterkopf präsenten Arbeit! Einen

speziellen Dank und Gedanken möchte ich hier meinem leider viel zu früh verstorbenen Opa Rudi Gehring widmen. Von ihm habe ich früh mitbekommen, wie wichtig Einsatz und harte Arbeit für den Erfolg sind. Gleichzeitig war er immer bescheiden sowie offen ehrlich interessiert für die Sorgen und Gedanken anderer Menschen eines jeden Kulturkreises: genau die Eigenschaften, die ein guter Wissenschaftler mit sich bringen sollte.

Ansonsten bin ich sehr dankbar für die finanzielle Unterstützung durch die DFG. Die Möglichkeit, im Rahmen des Graduiertenkollegs drei Jahre mit relativ wenig Belastung durch Lehre zu forschen, hat mir sehr geholfen. Als Teil des Graduiertenkollegs (RTG) bin ich auch unserer Sekretärin Christiane Fuhlrott zu Dank verpflichtet. Es war bestimmt nicht immer einfach, meine teils chaotischen Abrechnungen abzuwickeln, aber Christiane hat es immer geschafft. Danke auch an alle Betreuer und Professoren die Teil des RTG waren und sind: Die Kurse und das Feedback bei unseren RTG Treffen haben mir viel geholfen. Ein spezieller Dank nochmal an Stephan Klasen, der als Hauptverantwortlicher vor Ort für das gute Funktionieren des RTG gesorgt hat, und uns nach jedem Treffen mit detaillierten Notizen zu unserer Arbeit versorgt hat. Zu guter Letzt, Ehre wem Ehre gebührt, einen großen, großen Dank an meinen Doktorvater Axel Dreher. Ich weiß aus vielen Gesprächen mit anderen Doktoranden dass eine derart intensive und konstruktive Betreuung weit über das übliche Maß hinausgeht. Ich habe und freue mich immer noch sehr, dass ich im Endeffekt über eine Reihe an Zufällen zu dieser Chance gekommen bin. Und bin stolz auf mich, dass ich trotz anfänglicher Finanzierungsprobleme auf diese Chance hingearbeitet und Sie bekommen habe. Danke Axel, für diese außergewöhnliche Betreuung, und die vielen Chancen, die du mir ermöglicht hast!

Table of Contents

List of Tables and Figures

Introduction

While economics as a social science can be of tremendous importance to understand how societies and markets function, I was always particularly fond of political economy because of my interest in research questions that involve both economic and political issues and considerations. Moreover, the subfield public choice in particular succeeds in describing a more realistic view of the world by applying the same assumptions we economists usually use about private actors' motivations and utility functions to actors in the political sphere. My interest in this field was inspired by scholars such as James Buchanan and Gordon Tullock (e.g., 1962) or more modern work by for example Bruno Frey (e.g., 2010) or Bryan Caplan (2011).

Luckily economics as a science has deviated largely from modeling the behavior of "economic agents" in their varying roles from household members to market actors as purely rational homines oeconomici. We know now from authors like Daniel Kahneman (e.g., 2011, for a review see Shleifer 2012) and Amos Tversky (e.g., Kahneman and Tversky 1974), or economists like Ernst Fehr (e.g., Fehr and Fischbacher 2003) that both descriptions of human behavior as purely egoistic or purely altruistic cannot be accurate. Our actions are guided by a plethora of motives among which self-interest plays a crucial but not always decisive role. Moreover, assumptions like rationality and perfect information are useful for models that aim at carving out distinct features of economic interactions, but do not provide an accurate depiction of actual human behavior. The degree to which we act in a rational way depends to some extent on the importance we assign to a topic, on individual characteristics or external circumstances, but it is generally limited by our cognitive capacities and the biases to which we fall prey (see again Kahneman 2011).

The incorporation of these insights into economic modeling and thinking was one of the main changes in the profession in the 10-20 years, along with the rise of experimental economics (e.g., Karlan 2005, Ashraf et al. 2006, and Binmore and Shaked 2010). However, the degree to which they are implemented is dependent on the area of research, and could still not be taken for granted while I was undergraduate student. My aim when starting my dissertation was to incorporate these two ideas in my scientific work: All humans are prone to psychological biases and self-interest is usually one of their main motivations. This is of course always conditional on the specific circumstances and subject to heterogeneity. Applying this insight into economics helps in making our studies more realistic and more useful for society. However, we need to apply these assumptions consistently to all actors in the economy, including the political sphere. This is where I am influenced by public choice theory, which demonstrates that simply delegating decisions to central actors, in various more or less democratic ways, subject to some measure of preference aggregation, does not eliminate self-interest or cognitive biases. Hence, political actors individually, or collectively

in their role as governments and other political bodies, are also to some degree guided by their self-interest and prone to systematic biases.

All three studies contained in this dissertation, while covering different topics, apply this line of thinking. In the first paper of this dissertation, "Geopolitics, aid and growth" (Dreher Eichenauer and Gehring 2012), we take the re-election concerns and self-interested motivations of governments and their relation to the effectiveness of development aid into account. In the second paper, "Is there a home bias in sovereign ratings?"(Fuchs and Gehring 2013) we examine whether sovereign debt ratings by private credit rating agencies are subject to biases. The biases we analyze are based on political-economy factors like the self-interest of the agencies' home country governments and the institutional incentives, as well as cognitive biases on part of the rating agencies' analysts. In my third paper, "Crime, incentives, and political effort: A model and empirical application for India" (Gehring et al. 2015) we model how a factor like criminal background affects the incentives faced by incumbent politicians interested in re-election with regard to the political effort they engage in. The analysis incorporates asymmetric information between voters and their representatives and shows the extent to which accountability in the form of facing elections can succeed in setting the right incentives for elected politicians in such a framework.

All three studies are also concerned with topics of major political and economic importance that are politically highly controversial. I was always interested in controversial questions, and my aim is to apply sound economic reasoning and rigorous empirical, econometric methodology to help come to a deeper understanding of these questions. Luckily, the importance and appreciation of empirical research has increased over the last decade, along with an increased emphasis on the identification of causal effects. In this attempt we face an inevitable trade-off between internal and external validity. True randomization can, with very few exceptions, only be achieved in experimental settings under ideal circumstances. Many of the questions I am interested in cannot be answered in experimental settings, or the external validity of such endeavors would be doubtable. We are not (solely) interested in whether experimental subjects allocate aid due to altruistic considerations or self-interest, but also in what real world governments do. Similarly, our examination of biases in sovereign debt ratings cannot be replicated in a laboratory setting, as the underlying setting is much too complex.

Hence, my aim was to use econometric methods and innovative approaches based on sound theoretical considerations to produce studies that shed light on these important issues in a transparent and concise way. I always put an emphasis on a sound and comprehensive theoretical framework, formal or informal. In the paper on rating agencies we spend a considerable amount of space and time examining the potential causes for the biases that agencies are accused of, as well as months of background research about the actual decision-making process within the agencies. In my opinion, it is a crucial part of sound scientific

practice. Where applicable, like in the paper on the effort of parliamentarians, I also rely on formal theoretical models as an additional tool to demonstrate the relationships we want to emphasize and to provide a model that can also be applied to other related questions or countries in the future.

In terms of econometric approaches my mantra is to work as sophisticated as necessary, but to be as uncomplicated, open and clear as possible. The first paper uses a "natural experiment" to identify a causal difference between politically motivated and other development aid. In the paper on sovereign ratings, we rely mostly on extensive controls based on comprehensive reviews of the literature and the rating agencies own publications, as well as on fixed effects regressions to account for remaining unobserved variation. Heckman and quantile regressions serve as robustness checks and to further disentangle the channels and relationships we discover. In my last paper, I apply matching techniques, endogenous selection models based on switching regressions and methods to assess the extent to which remaining selection-on-unobservables can affect coefficient estimates. Opinions on econometric methods diverge and are always subject to discussion; my intention - and what I tried hard to achieve - was to apply the appropriate methods to the respective question under examination and be transparent about the methodology.

I will summarize the content of these three most important papers during my PhD on the next pages:

Part 1: Geopolitics, Aid and Growth

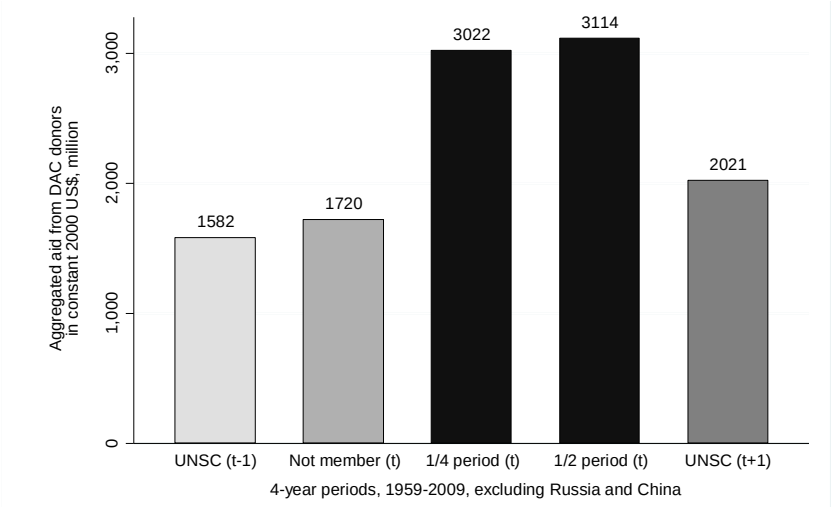
One of the main challenges in the world today continues to be eliminating extreme poverty and enabling poor countries to develop. Arguably one of the major tools to achieve this goal is to provide monetary support to developing countries either via international organizations like the World Bank, bilaterally between countries, or via private charity or other non-governmental organizations (NGOs). The bulk of these aid flows in the last centuries since 1961 has been registered and accounted for via the OECD's Development Assistance Committee (DAC). The official and concessional part of these flows was defined as "official development assistance" (ODA) in 1969, and since then has been used as the main measure to evaluate aid allocation and performance.

While aid is partly aimed at fostering other goals like bridging short-term food shortages or health problems as well, the major goal arguably is to increase the long-term development of recipients. There are a large number of studies examining whether development aid by (mostly) Western countries succeeds in promoting growth in developing countries. Doucouliagos and Paldam (2009) present a meta-study of these attempts, which comes to the conclusion that, on average, there is a zero or only marginally positive effect of ODA on growth rates.

Whether and to what extent the motivation to allocate aid actually stems from an underlying altruistic preference for helping those in need and improving their situation, or instead is used to achieve other political and economic targets of the donor country is a crucial question to evaluate this finding. A political-economy perspective on aid allocation would suggest that donors that are, at least partly, maximizing their or their voters' self-interests, would also take these into account when allocating aid. In fact, there is a large literature documenting the impact of political and economic considerations on aid allocation. Starting with Kuziemko and Werker (2006), multiple studies documented how donors use aid also to support their own political agenda, benefit domestic companies, or channel funds to political allies.

We investigate the effects of short-term political motivations on the effectiveness of foreign aid. Specifically, we test whether the effect of aid on economic growth is reduced by the share of years a country has served on the United Nations Security Council (UNSC) in the period the aid has been committed, which provides quasi-random variation in aid. Figure 1 from the study gives a first overview of this variation in aid disbursements. The data are divided into four year periods. It can be seen that if a country spends one or two years as a member of the UNSC during a period, aid commitments nearly double. The decline after being a member indicates, along with existing research, that this variation is really caused by being a UNSC member.

Figure 1: Total aid commitments and temporary UNSC membership (t)



Without diving into the detailed econometric framework here, we assume that this bulk of aid consists of mostly politically motivated aid, or more specifically, to a larger degree of politically motivated aid. The relationship we examine assumes that increased commitments while being a member lead to increased disbursements in the following period. We then measure whether these increased disbursements have a different, lower or more negative, relation to growth rates in the recipient country. Our results show that the relationship of aid with growth is significantly lower when aid has been committed during a country's tenure on the UNSC.

Part 2: Is there a home bias in sovereign ratings?

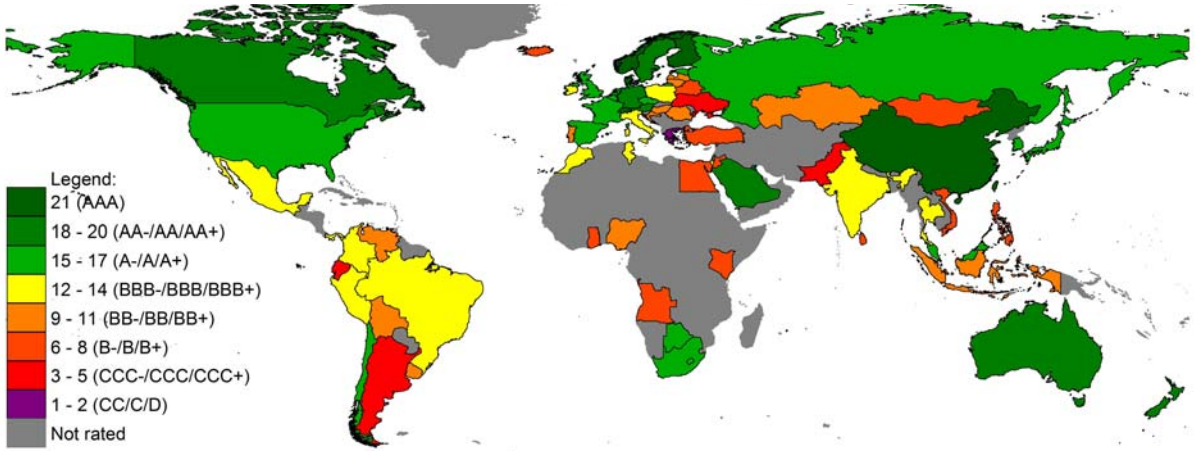
The actions of credit rating agencies have come under intense scrutiny – in particular during the recent Global Financial Crisis and the current sovereign debt crises in Europe. The provision of accurate and unbiased sovereign ratings is crucial for the global economy as rating decisions affect the borrowing costs of states (e.g., Afonso et al. 2012) and set de-facto ceilings to corporate ratings (e.g., Borensztein et al. 2013). However, many scholars and policymakers around the world blame rating agencies for unreliable practices, unfortunate timing and misjudgments. Russia's president Vladimir Putin and Germany's finance minister Wolfgang Schäuble speak of "abuses" and "abusive behavior," Turkey's premier Recep Tayyip Erdoğan makes claims of "unfair" decisions, and José Manuel Barroso, president of the European commission, directly accuses the agencies of a "bias […] when it comes to the evaluation of specific issues of Europe." We thus ask the question whether empirical evidence supports this idea of a systematic bias in sovereign ratings.

Ideally, competition and concerns over reputation should incentivize agencies to publish accurate and unbiased ratings. However, as Bernhard Bartels and Beatrice Weder di Mauro have argued in a Voxeu column (http://www.voxeu.org/article/rating-agency-europe-good-idea), flawed models, bad incentives, and the concentrated market structure distort competition in the rating business and can lead to biased rating decisions. Many of the concerns about biased sovereign ratings revolve around the role of the credit rating agency's "home country" – a factor largely disregarded in previous research. We define a "home bias in sovereign ratings" as a deviation of the rating level in favor of the home country (or countries aligned with it) from what would be justified by the sovereign's economic and political fundamentals. By analyzing a broad set of rating agencies, our recent paper (Fuchs and Gehring 2013) investigates whether there is systematic evidence for such a bias and analyzes the factors that might be driving it. While most of the variation in sovereign ratings is explained by the fundamentals of rated countries, our results provide empirical support for the existence of such a home bias, which is driven by the home country's economic interests as well as cultural ties between home and sovereign.

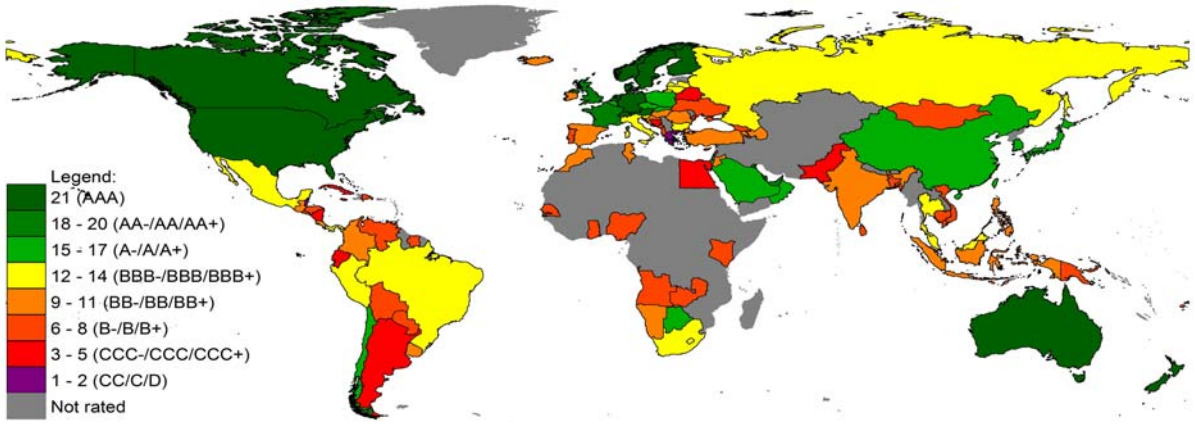
Of the current 150 credit rating agencies, most agencies are only active in narrow national or regional markets and focus solely on corporate ratings (White 2010; De Haan and Amtenbrink 2011). In addition to the three big U.S.-based agencies, Fitch, Moody's and S&P, there are six other agencies, based in five different countries, that provide sovereign ratings for at least 25 sovereigns: Capital Intelligence (Cyprus), Dagong (China), DBRS (Canada), Feri (Germany), Japan Credit Rating Agency, and Rating and Investment Information (Japan). While the U.S.-based agencies dominate the market, the interest in these smaller agencies is increasing – not only in the financial sector. Downgrades by Dagong in particular receive a lot of media attention. Figure 2 illustrates the rating process, which is similar for all agencies.

A simple comparison of the sovereign ratings issued by China-based Dagong and U.S.-based Moody's illustrates why ratings might be perceived as influenced by the agency's home country (see Figure 4). Compared to Moody's, Dagong assigns higher ratings to the Chinese territories Hong Kong and Macao as well as to the group of BRICS countries, including China itself. At the same time, Dagong assigns lower ratings to many Western economies than Moody's does.

Figure 2: Sovereign rating maps for Dagong and Moody's



Dagong (China)



Moody's (USA)

We argue that a home bias in sovereign ratings can be the result of both political economy influences and culture. With respect to political economy influences, governments have strong reasons to sway rating outcomes: Ratings determine a sovereign's access to international capital markets and its borrowing costs. Home-country governments possess substantial leverage over agencies as they control the respective regulatory body whose official recognition agencies need to operate (White 2010; De Haan and Amtenbrink 2011). Since ratings provided to home-country firms are a major source of income, the prospect of losing that recognition is an imminent threat to agencies. Anecdotal evidence points at alleged attempts of intimidation against US agencies by the US government and at strong relations of China-based Dagong with the Chinese government (see Fuchs and Gehring 2013 for details).

The second mechanism could be culture: Various studies show that cultural distance affects financial decision-making of both households and firms (e.g., Grinblatt and Keloharju 2001). Giannetti and Yafeh (2012), for example, show that bank professionals grant smaller loans and charge higher interest rates to banks that are culturally more distant. Given this evidence, it would not be surprising if cultural distance also affects decision-making at rating

agencies. Our empirical results in Fuchs and Gehring (2013) show that, for the nine agencies studied; 86% of the variation in sovereign ratings can be explained by "objective" sovereign-specific economic and political fundamentals. Nevertheless, we find evidence of a home bias in sovereign ratings.

The results show that the average agency assigns a rating to its home country that is almost one point higher than justified by how they assess other sovereigns. We also find that countries with a higher level of cultural proximity, as operationalized by measures of linguistic distance, receive better treatment. If this cultural bias was solely due to a lack of information, the existence of an office in a rated country should alleviate it. When we interact the existence of an office with cultural distance, however, the bias is not mitigated. Thus, the most plausible explanation is that cultural distance relates to more pessimistic risk perceptions. Previous studies have shown that predictions about future developments are more optimistic when they refer to the home country (e.g., Kilka and Weber 2000) and that cultural distance is negatively related to bilateral trust (Guiso et al. 2009). Bilateral trust not only affects how the available information is perceived, but also affects beliefs about a sovereign's willingness to repay its debt. History has shown that countries commonly default on their debt for reasons that are opportunistic and relate to domestic political considerations rather than insufficient liquidity.

Part 3: Crime, incentives, and effort- A model and empirical application for India

Criminal or corrupt politicians can pose a serious problem to the functioning of democratic systems. In India, the world's largest democracy, the issue of criminals in politics is particularly severe. In the lower house of the Indian parliament, the Lok Sabha, about 34% of the members of parliament (MPs) face criminal charges as of 2014. These charges include severe crimes like murder and rape. Already the subject of popular novels like "The White Tiger"[1], a whole strand of economic and political science literature is emerging as well, to which we contribute. While India is a particularly drastic example, other countries like Brazil face similar problems (cf. Ferraz, Claudio and Frederico Finan, 2008).

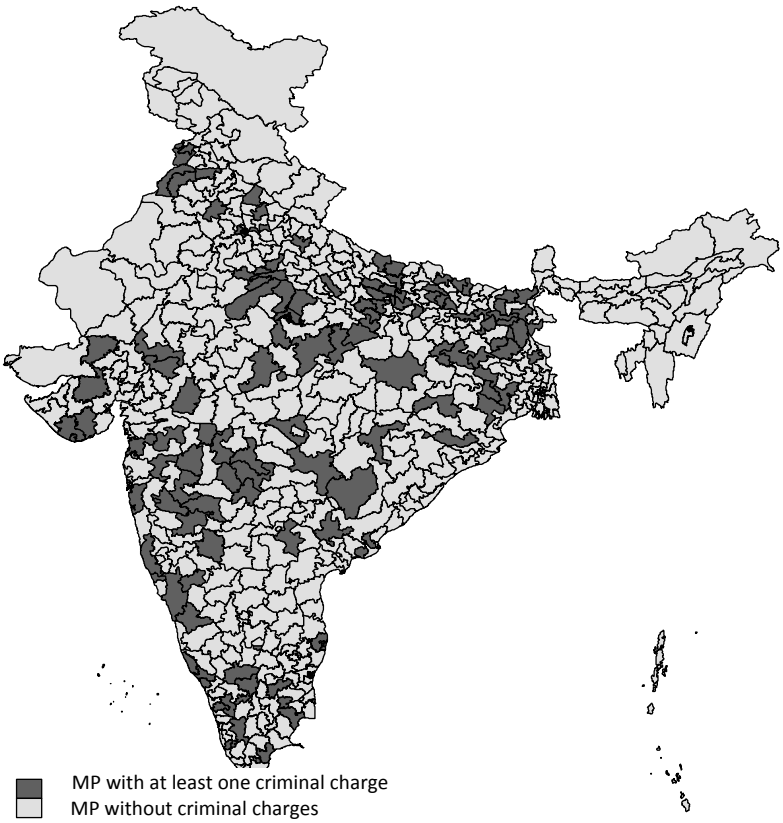
Our general formal theoretical model focuses on the relationship between incumbents and voters. MPs are aware that a higher effort level increases the probability of being reelected, but they lose utility from time allocated to political work. Thus, there is a tradeoff between

[1] Novel by Aravind Adiga, available for example at http://www.amazon.com/The-White-Tiger-A-Novel/dp/1416562605. It describes how a village boy, with initially good motives, is pulled into a circle of violence and criminal actions on his way from rags to riches. It portrays the country's issues with criminality, corruption and violence in politics and the society in general with a dark sense of humor.

utility and the chance of being reelected. Furthermore, MPs need to take the other determinants of voting decisions into account when choosing their effort level. Using comparative statics with respect to the model parameters we demonstrate, for instance, that the effort of criminal MPs should be relatively more when they contest in a more developed electoral district. In doing so, we connect to the emerging literature that examines electoral competition in the context of India. For instance, Aidt et al. (2015) investigate why parties field criminal candidates and Dutta and Gupta (2012) analyze competition between candidates that include criminals. In their seminal paper, Besley and Burgess (2002) model incumbents of different unobservable types who can exert effort to help voters in need. Since we focus on the relationship between incumbents and voters, our model is complementary to the first two papers and, in contrast to Besley and Burgess (2002), focuses particularly on the behavior and characteristics of criminal incumbents.

In order to empirically test our hypotheses we use details about the candidates' criminal records available thanks to a 2003 Indian Supreme Court judgment that made it mandatory for every candidate contesting state and national elections to provide sworn affidavits. These include details about their not only personal, educational, and financial particulars, but also detailed information on any criminal charges they had faced, the status of their criminal cases and any charges against them. We use criminal charges as a signal whether an MP is a criminal type. Using data for the 14th Lok Sabha allows us to control for confounding factors such as past electoral performance or party strongholds, which are crucial for MP effort. Figure 3 shows the geographical allocation of the MPs with and without criminal charges across India.

Figure 3: Geographical distribution of candidates with criminal charges across constituencies for the 14th Lok Sabha (after 2004 national election)



The observation levels are the 543 constituencies, which each elect one MP in a first-past-the-post-system. We find that across specifications, criminal background is related to around 5% higher absenteeism rates. Parliamentary activity, on the other hand, does not differ significantly between allegedly criminal and non-criminal MPs in our baseline specification. As suggested by our model, alleged criminal MPs show both significantly lower attendance rates and less parliamentary activity in constituencies that are economically underdeveloped. The reason could lie in better monitoring in the more developed constituencies and/or the greater attractiveness of these constituencies for criminals in terms of rent-extraction possibilities. The coefficients become more negative when we use an alternative indicator that should alleviate concerns about measurement error instead. The Member of Parliament local area development scheme (MPLADS) utilization is lower for criminal MPs in general, but only significant for the alternative indicator.

As part of our strategy to identify the causal effect of criminal type on the outcome variables, we first use the model as a theoretical foundation to derive an extensive set of relevant constituency- and MP-specific control variables. Second, fixed effects for major states ensure that the results are not driven by factors specific to certain Indian regions such as, for example, economic underdevelopment. Fixed effects for major parties ensure that the coefficient estimates are not driven by unobserved factors specific to a party or related to

being part of the government or opposition. Third, we get identical results using alternative matching estimators and show that the control and treatment groups are strongly balanced. Results from treatment effect estimations that model the selection process explicitly yield slightly more negative estimates. For example, when controlling for selection, both indicators of criminal type are significantly related to lower development fund utilization rates. This suggests that, if anything, omitted variables and/or selection effects seem to bias our coefficients for criminal background towards zero. For the negative relation between one criminal indicator and MPLADS utilization rates, we use selection-on-observables to demonstrate why this is, under relatively mild assumptions, an upper bound estimate. Moreover, we use methods developed in Altonji et al. (2005) to demonstrate that on average selection bias (on unobserved factors) would have to be between two and sixteen times greater than selection on observed factors to fully explain the negative relation between and attendance rates or MPLADS utilization. As we have plausibly identified the most relevant influencing factors in our model, it seems that criminal MPs in India indeed have a detrimental impact on their constituency.

I Geopolitics, Aid and Growth

Coauthors:
Axel Dreher (Heidelberg University, University of Goettingen, KOF, CEPR, CESifo, IZA)
Vera Eichenauer (Heidelberg University)

Abstract: We investigate the effects of short-term political motivations on the effectiveness of foreign aid. Specifically, we test whether the effect of aid on economic growth is reduced by the share of years a country has served on the United Nations Security Council (UNSC) in the period the aid has been committed, which provides quasi-random variation in aid. Our results show that the relationship of aid with growth is significantly lower when aid has been committed during a country's tenure on the UNSC. We derive two conclusions from this. First, short-term political favoritism reduces the effectiveness of aid. Second, political interest variables are inadequate as instruments for overall aid, raising doubts about a large number of results in the aid effectiveness literature.

JEL codes: O19, O11, F35, F53

Keywords: Aid effectiveness, Economic growth, Politics and aid, United Nations Security Council membership, Political instruments

Acknowledgements: We thank Kurt Annen, Jean-Paul Azam, Sarah Bermeo, Simone Bertoli, Christian Conrad, Angus Deaton, Hristos Doucouliagos, Matthias Hartmann, Jude Hays, Richard Jong-A-Pin, Philip Keefer, Jamus Lim, Katharina Michaelowa, Daniel Nielson, Martin Paldam, James Snyder, Alois Stutzer, Byungwon Woo, Isleide Zissimos, participants at the 6th Annual Conference on The Political Economy of International Organizations (Mannheim/Heidelberg 2013), the Second Warwick Political Economy Workshop (2013), the Beyond Basic Questions Workshop (Lucerne 2013), the European Public Choice Society Meeting (Zurich 2013), the Conference on Research Frontiers in Foreign Aid (Princeton 2013), the Spring Meeting of Young Economists (Aarhus 2013), the Schliersee Workshop on "Public Finance and Political Economy I," the 2013 Silvaplana Workshop in Political Economy, the 2013 annual meeting of the International Political Economy Society at Claremont Graduate School, the Northeast Universities Development Consortium Conference 2013 at Harvard University, the EUDN Scientific Conference 2013, the Future of Political Economics (Freiburg 2014), the Verein für Socialpolitik (Hamburg 2014), and at seminars at the Instituto Universitário de Lisboa (ISCTE-IUL), University of Milano-Bicocca, University of Auvergne (CERDI), University of St. Gallen, and Heidelberg University for helpful comments. We thank Jamie Parsons for proofreading. Financial support from the German Research Foundation (DFG GZ: DR 640/2-2) is gratefully acknowledged.

"UNSC membership offers a quasi-experiment to assess the impact of unconditional aid."
(Bueno de Mesquita and Smith 2010)

1. Introduction

For a new paper investigating the impact of aid on economic growth it may be good practice to begin with an apology for adding to such an immense literature. However, the debate on whether or not foreign aid is effective in promoting growth in recipient countries is ongoing and heated, arguably because the literature lacks an accepted identification strategy. While we do not offer recipes to estimate a causal effect of aid on growth, we propose a test to distinguish between the effects of aid committed while a country benefited from extraordinary political leverage and the effects of aid committed at other times. While the effect of favoritism on how aid promotes growth is interesting in its own right, our paper also offers important insights for those studies in the aid effectiveness literature that use political alignments to identify the effects of aid.

Most of the previous literature relies on three strategies to identify the effect of aid on growth (or other potential consequences of aid).[2] First, researchers use instruments for aid that mainly rely on the recipient country's population size. Second, they employ internal instruments in the context of difference or system GMM estimations. Third, they base the analysis on instruments that proxy for the geopolitical importance of a recipient country to the donor, implicitly or explicitly generalizing the Local Average Treatment Effect (LATE) to be representative of all aid, rather than political aid exclusively.[3] The first two estimation

[2] A number of recent contributions does not fit these groups. For example, Galiani et al. (2014) instrument aid flows with the International Development Association's (IDA) threshold for receiving highly concessional aid. Werker et al. (2009) make use of oil price fluctuations that substantially increase the aid budgets of oil-producing Arab donors, in particular to Muslim countries. While we are convinced of Werker et al.'s identification strategy, in particular, their results can hardly be generalized to represent the effects of aid more broadly. As they point out, their results show the LATE for oil-price-induced increases in aid to Muslim countries, which might be unrepresentative for aid by a broader set of donors to a broader set of recipients. The results in Galiani et al. (2014) refer to the small set of countries crossing the IDA-threshold.

[3] The number of papers falling in this category is too large to cite them all. A number of recent papers use (changes in) voting alignment between the donor and recipient in the United Nations General Assembly (e.g.,

strategies violate the exclusion restriction. Clearly, population size and lagged aid can affect growth through channels other than contemporaneous aid.

The third strategy requires assuming that the effects of aid are independent of the donors' motives for granting it. This might be reasonable. Donors who have already committed a certain amount of aid might be keen to achieve developmental outcomes, independent of the motive for granting aid in the first place (Rajan and Subramanian 2008). Kilby and Dreher (2010) raise doubts about this homogeneity assumption. Their results show that donor motives influence the effectiveness of development aid in promoting growth.[4] Several reasons might explain such a difference. Arguably, if donors are motivated by pure self-interest, their allocation decision does not depend on the way the recipient uses the aid. A politically motivated allocation of aid may result in the approval of lower-quality aid projects in favored countries instead of more promising projects elsewhere. Donors may fail to include growth-promoting policy conditions or wave them in case of non-compliance. Favoritism might thus allow projects to be pursued where important preconditions are not met or might reduce the time and resources devoted to the preparation of a project. The recipient might choose to use disbursed aid for purposes other than development if punishment for non-compliance is less likely,[5] resulting in on average inferior outcomes.

Creasey et al. 2015, Aurore and Maurel 2013, Bjørnskov 2013, Midtgaard et al. 2013). Others use temporary membership in the United Nations Security Council, which is in the focus of our paper (Christensen et al. 2011, Breitwieser and Wick 2013, Drometer 2013).

[4] They do not propose exogenous instruments to identify causality, however, but rely on system GMM estimations (with the exclusion restriction being unlikely to hold, see Bazzi and Clemens 2013). A handful of other studies consider the impact of donor characteristics on aid effectiveness (Bobba and Powell 2007, Headey 2008, Bearce and Tirone 2010, Minoiu and Reddy 2010, Bermeo 2011).

[5] Focusing on the IMF and the World Bank, Stone (2008), Kilby (2009) and Nooruddin and Vreeland (2010) suggest that political favoritism undermines the credibility of conditionality, rendering it ineffective. Nooruddin and Vreeland (2010) show that democratic countries under IMF programs increase public wages and salaries when they serve on the UNSC, while governments without UNSC-related political leverage have to reduce their wage bill. This suggests that politically important countries can avoid tough conditionality. Stone (2004) and Kilby (2009) show that IMF and World Bank conditions, respectively, are not rigorously enforced in politically important recipient countries (measured by UNGA voting patterns, among others). Kilby (2011, 2013) finds that political leverage reduces the time used to prepare World Bank projects, which in turn reduces the quality of these projects.

If geopolitical aid or aid given to recipients with political leverage more generally is less effective than other aid,[6] the literature using political connections as instruments would not provide evidence of the ineffectiveness of overall aid, but rather of aid given to politically important countries. Their estimates would represent a lower bound for the effects of overall aid. The lack of a convincing test for differential effects of these types of aid is thus an important gap in the literature. In this paper, we aim to fill this gap.

We investigate whether aid given to temporary members of the UNSC is less effective in promoting growth than aid given at other times. In measuring the amount of aid received by a country at times of extraordinary political importance, we connect to the recent literature investigating the effects of temporary membership on the UNSC. Bueno de Mesquita and Smith (2010) show that temporary members grow more slowly while serving on the UNSC. They attribute this to the adverse consequences of development aid, given that these temporary members receive substantial additional inflows of aid during their terms on the UNSC (Kuziemko and Werker 2006, Dreher et al. 2009a, 2009b). However, the results in Bueno de Mesquita and Smith reflect the effects of membership per se, and seem to be independent of the amount of aid received (Bashir and Lim 2013).[7] It thus remains unanswered whether aid committed during temporary UNSC membership results in different developmental outcomes than aid given at other times.

Dreher et al. (2013) investigate the effect of temporary UNSC membership on the evaluation of World Bank projects. Their results show that project evaluations are on average not inferior for projects committed to countries while being on the UNSC. It is only during times of macroeconomic crisis that politically motivated aid reduces the probability of a positive evaluation. In contrast to our paper, Dreher et al. focus on one (multilateral) donor and investigate the effect of geopolitics on self-assessed project outcomes rather than on economic growth. We take a broader approach and reconsider recent models of aid

[6] Overall aid is composed of an (unknown) share of politically motivated aid and, arguably, some share exclusively given for developmental purposes, among others. See Werker (2012) for more on the political economy of foreign aid.

[7] As pointed out by Bueno de Mesquita and Smith (2013), the effects of "easy money" can take many routes, among them, as they show, loans to the temporary UNSC members.

effectiveness to distinguish between aid given to countries of short-term geopolitical importance and aid committed at other times. In contrast to Bueno de Mesquita and Smith (2010), we do not relate UNSC membership per se to the variables of interest, but exploit the variation in the amount and the implementation modality of aid at the time of temporary UNSC membership to investigate whether the effectiveness of these flows is different from aid committed at other times.

We run first-difference and two-way-fixed effects regressions for a maximum of 119 countries and the 1970-2005 period. Augmenting Clemens et al.'s (2012) permutations of Burnside and Dollar (2000) and Rajan and Subramanian (2008), we find that the effect of aid on growth is reduced by donors' geopolitical motives. This result holds for the model of Bueno de Mesquita and Smith (2010). It is more pronounced in autocratic recipient countries and holds if we restrict the sample to Africa, which follows the strictest norm of rotation on the UNSC and can thus most reliably be regarded as exogenous. Overall, we find that political favoritism reduces aid effectiveness. This renders political variables inadequate as instruments for overall aid.

The next section describes how we exploit temporary membership on the UNSC to identify the effects of political motives, outlines our data and method of estimation, and presents the main results. Section 3 extends the basic analysis, while the final section draws policy implications and concludes the paper.

2. Data, method and main results

Our proxy variable for political importance is a measure that has been shown in previous research to induce political favoritism: temporary membership on the UN Security Council. Among the potential proxy variables for political influence, this measure poses the fewest problems.[8] Membership positions are scarce, the nature of service is temporary and not

[8] Other measures suggested in the literature include voting patterns in the UN General Assembly, formal alliances or military support, colonial relationships, stronger geopolitical constraints during the Cold War period compared to more recent years, and ad hoc classifications of "good" versus "other" or "bad" donors. None of

immediately renewable, and the selection process is exogenous to aid (Bueno de Mesquita and Smith 2010, Dreher et al. 2014).[9]

While five members of the UNSC (China, France, Russia, the United Kingdom, and the United States) serve on a permanent basis, ten temporary members are elected by the UN General Assembly. These elected members serve two-year terms. While not random, membership appears to be largely idiosyncratic, with varying regional norms (Dreher et al. 2014): African nations typically rotate; Latin America and Asia hold competitive elections where regional hegemons win most often; Western Europe mixes rotation and competitive elections; and since the end of the Cold War, Eastern Europe shows no systematic pattern. The two-year, not immediately-renewable term reinforces the exogeneity of the selection process.

Our analysis closely follows the approach in Clemens et al. (2012), adding our variables of interest to their models. Clemens et al. show that the most prominent previous attempts to control for the potential endogeneity of aid rely on invalid instruments.[10] Instead of suggesting more valid ones, they address the potential endogeneity of aid by differencing the regression equation and lagging aid, so that it can reasonably be expected to cause growth rather than being its effect. Thus, they assume that the main (short-term) effects of aid on growth occur, on average, one period after its disbursement. We base our analysis on

---

these measures is suitable to identify causal effects of politically motivated aid given that they vary little and slowly over time, so that most of the variation in these measures comes from the cross-sectional dimension.

[9] For our work, the importance of previous research on what determines election to the UNSC cannot be over-emphasized. If selection to the UNSC depends on those same variables that also affect aid and economic growth, our results would be biased. While countries become politically or economically more important over time, the amount of aid they receive and their rates of economic growth could simultaneously increase. For example, countries being temporary members of the UNSC might be able to draw the world's attention to their legitimate developmental needs, giving them access to additional funds that are unrelated to political motives. Bueno de Mesquita and Smith (2010) and Dreher et al. (2014) test this possibility: They find that election to the UNSC is clearly not related to the variables that also affect the amount of development aid a country receives. Thus, conditional on the variables in our models, UNSC membership can be considered as providing exogenous variation that we can use to identify the temporary geopolitical importance of a country for exactly its two years of membership. See also Besley and Persson (2012).

[10] Bazzi and Clemens (2013) show in more detail that previous papers in the aid effectiveness literature rely on weak instruments – including those relying on internal instruments using "black box" GMM estimations.

their permutations of Burnside and Dollar (2000) and Rajan and Subramanian (2008) – the two studies that arguably gained most attention in the recent literature on aid and growth. We also re-estimate the specifications in Bueno de Mesquita and Smith (2010), which are closely related to the question we address here, and which gained considerable attention in the academic literature and the media alike.[11] While we believe (as do Clemens et al. 2012) that OLS regressions are superior to 2SLS with questionable instruments, we stress that our estimate of whether aid affects growth could be biased in either direction,[12] and we largely refrain from interpreting the aid-growth relationship as causal. There is, however, no reason to expect a systematic bias for our coefficient of interest, the interaction of aid with UNSC membership for any given level of aid.[13] We thus adopt the regression-based OLS approaches of these prominent previous analyses,[14] and add development aid and its interaction with temporary membership on the UNSC to the equations of Bueno de Mesquita and Smith (2010), and temporary membership and its interaction with aid to those of Clemens et al. (2012).[15]

In terms of timing, we follow Clemens et al.'s (2012) "Burnside and Dollar"-regressions and assume that disbursed aid takes one four-year-period to become effective to increase or decrease economic growth.[16] We also assume that bottlenecks in the donor and recipient

[11] E.g., Hosli et al. (2011), Bashir and Lim (2013).

[12] For example, donors might grant more aid to a new reform-oriented government. Increased growth resulting from these reforms could then spuriously be attributed to the increases in aid. On the other hand donors might give more aid to countries where they anticipate shocks to reduce future growth rates.

[13] This interpretation relies on the formal analysis in Nizalova and Murtazashvili (2012). Also see Nunn and Qian (2013). Nunn and Qian refer to section 2.3.4 of Angrist and Krueger (1999) for a technical discussion.

[14] Unlike Clemens et al. and Bueno de Mesquita and Smith we cluster standard errors at the recipient country level in all our specifications. Our results are not affected by this.

[15] As an alternative approach, one could think of instrumenting for aid with temporary membership on the UNSC. We do not pursue this route for two reasons. First, temporary membership is rare – the instrument thus has low power. More importantly, instrumenting aid with UNSC membership can only give us the LATE – in this case, the effect of aid given while a country was of short-term geopolitical importance. However, we are interested in the difference of the effectiveness of strategic aid compared to aid given at other times.

[16] As summarized in Headey (2008), aid affects growth most substantially 5-9 years after it has been disbursed, on average. If aid is disbursed evenly over time, the average positive distance between a dollar being disbursed and growth in the contemporaneous four-year-period is 16 months (Roodmann 2007, Headey 2008). Headey thus lags aid by one four-year period, so that the average positive distance between disbursements and their potential effects is 5 years and 4 months.

administrations prevent aid from being disbursed immediately, so that the bulk of aid committed in one four-year-period is disbursed one period later, on average.[17] Based on these assumptions about the lag structure of the growth effects of aid we are interested in growth rates two periods after UNSC membership. Regarding the potentially harmful consequences of geopolitical motives, this would imply that aid committed in period (t), which is disbursed in period (t+1), is less effective in promoting growth in period (t+2) the more years a country has spent on the UNSC in period (t). Arguably, UNSC membership can also have more instant or contemporaneous effects on growth, depending on the exact channel that explain the reduced aid-growth correlation. We empirically test the possibility of different timings in a series of additional regressions.

Figures 1-3 provide a first impression of the data. The patterns are in line with our assumptions about the most likely timing. Figure 1 shows that total aid commitments (in constant 2000 million US dollars) from all DAC-donors in a specific four-year-period are substantially larger for countries that have served (one or two years) on the UNSC, compared to countries that did not serve. They are also larger compared to commitments received by temporary members in the period prior to serving on the UNSC, and compared to commitments one period after serving (these differences are statistically significant at the one-percent level). Figure 2 shows net aid disbursements (also in constant 2000 million US dollars) conditional on UNSC membership, but lags membership by one four-year-period. The data support the assumed pattern: Commitments increase in the contemporaneous four-year-period of membership; the accompanying disbursements increase in the period following UNSC membership. Thus, aid commitments during UNSC membership seem to be disbursed on average one period later. Both commitments and disbursements move back to their initial levels in periods (t+1) and (t+2) respectively. Overall, the effects coincide with UNSC membership, and disappear after the temporary member loses its extraordinary geopolitical importance.

[17] For example, a 1999 report of the British House of Commons' Select Committee on International Development reports a delay between European Commission aid commitments and disbursements at the end of the 1990s of almost five years (cited in Odedokun 2003: 7). See OECD (2003) for an in-depth discussion of reasons for delayed disbursements.

Figure I.1: Total aid commitments and temporary UNSC membership (t)

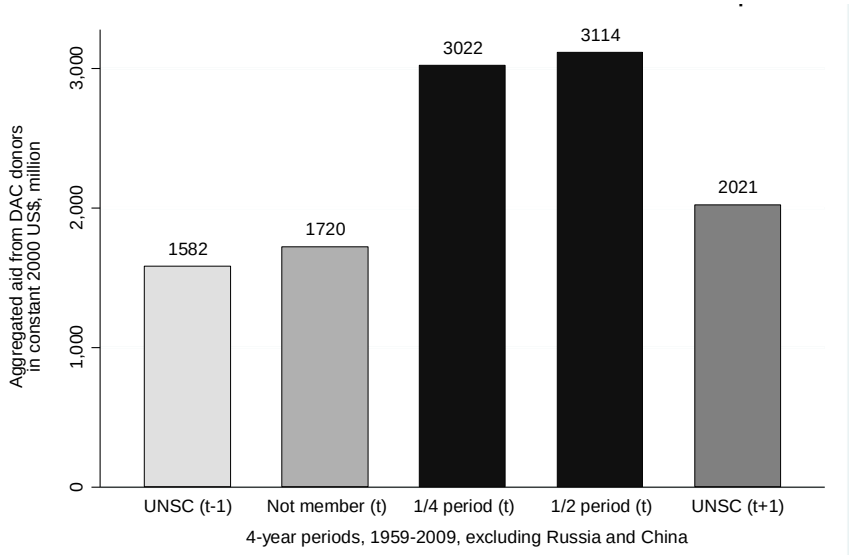


Figure I.2: Total aid disbursements and temporary UNSC membership (t)

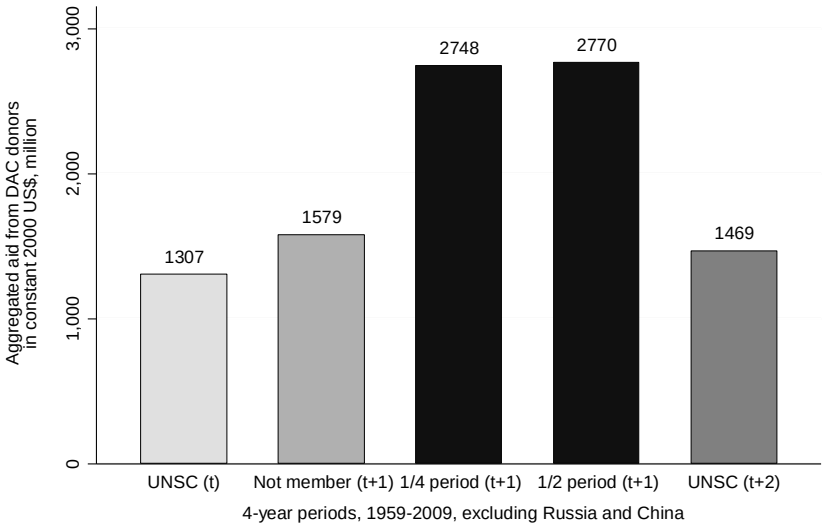
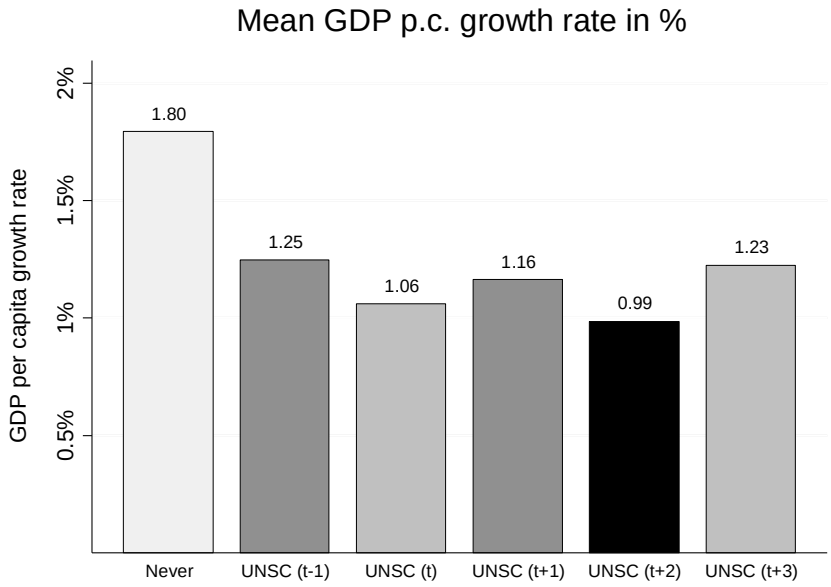


Figure 3 shows mean yearly growth rates of per capita GDP for different lags of UNSC membership. The first bar displays the growth rates for countries that have never been a member of the UNSC. The other bars show the growth rates for different lags of UNSC membership: Growth during UNSC membership, one period before, one period later, two periods later, and three periods later. The figure supports the notion that compared to countries that have never served on the UNSC, UNSC members subsequently experience

lower growth rates. That is, in line with Bueno de Mesquita and Smith (2010), we find that UNSC membership correlates with lower growth rates. Still, as expected, the lowest growth rates are experienced two periods after UNSC membership. Also note that growth rates increase to almost the level of the pre-UNSC period in the period after UNSC membership. It thus seems that the commitments made while being on the UNSC are not disbursed in sufficient amounts in the next period, on average, to substantially decrease growth in that period.

Figure I.3: GDP per capita growth and temporary UNSC membership (t)

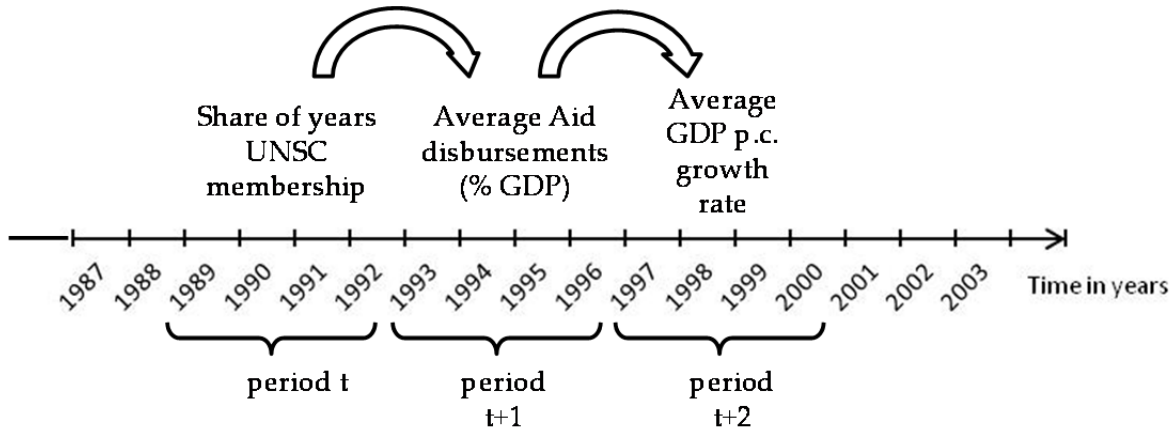


This pattern supports our hypothesis that the increased aid committed in period (t) during temporary UNSC membership [Figure 1], which is disbursed in large parts in period (t+1) [figure 2], has an adverse effect on growth in period (t+2) [figure 3].[18] While these descriptive

[18] When we directly test the effect of aid commitments rather than disbursements in our models below, we find no significant effect on growth. Commitments are not usually used in the aid effectiveness literature, as only those parts of the commitments actually disbursed can affect the outcome variable. We also looked at the correlation between disbursements and commitments and find them to be higher for temporary UNSC members than for non-members. This shows that the extent to which the respective commitments are actually disbursed is lower for non-UNSC members than for UNSC members. The blurred distinction between UNSC and non-UNSC members makes it less likely to find significant effects when using commitment data.

statistics imply no causality, their pattern lends support to our story. We illustrate the timeline derived from our considerations in figure 4. We test different timings as a robustness check further below.

Figure I.4: The proposed timeline



Next we turn to our econometric specifications. Following Clemens et al. (2012) our reduced-form empirical model is at the country-period level:

$$Growth_{i,t} = \alpha+\beta Aid_{i,t-1}+\gamma Aid^2_{i,t-1}+\delta UNSC_{i,t-2}+\zeta Aid_{i,t-1}{}^{*}UNSC_{i,t-2}+\eta X_{i,t}+\varepsilon_{i,t} \quad (1)$$

where $Growth_{i,t}$ is a country i's average yearly real GDP per capita growth over period t. $Aid_{i,t-1}$ denotes the amount of aid (as a percentage of GDP)[19] disbursed in the previous period; $UNSC_{i,t-2}$ indicates the share of years country i was a temporary member of the UNSC two

[19] We focus on aid from all donors for two reasons. First, UNSC membership has been shown to be important for the allocation of aid from most of the largest donors (see Vreeland and Dreher 2014 for an overview). Given that these donors account for the bulk of aid we do not want to exclude some donors on an ad hoc basis. To the extent that these donors do not provide more aid to countries on the UNSC this does not bias our results. Second, aid by single donors, or a subset of them, is usually not sufficiently large to be measurable in terms of growth. Still, we replicated our results focusing on aid from the largest donor – the United States – separately, as we describe in more detail in footnote 35.

periods before.[20] We expect that aid commitments are, on average, disbursed one period later. Hence, when using lagged aid we twice-lag temporary membership on the UNSC ($UNSC_{i,t-2}$). All regressions include the complete set of contemporaneous control variables used by the respective previous studies, which is denoted $X_{i,t}$.[21] Our preferred specification also includes aid squared to test for decreasing returns to aid, again following Clemens et al. (2012).[22] Finally, $\varepsilon_{i,t}$ is the error term.

Equation (1) is in levels and thus does not address the potential endogeneity of aid to economic growth. We therefore base our conclusions mainly on a regression in first differences to control for time-invariant omitted variables, as in Clemens et al. (2012).[23] Equation (1) then becomes:

$$\Delta Growth_{i,t} = \alpha+\beta\Delta Aid_{i,t-1}+\gamma\Delta Aid^2_{i,t-1}+\delta UNSC_{i,t-2}+\zeta\Delta Aid_{i,t-1}*UNSC_{i,t-2}+\eta\Delta X_{i,t}+\varepsilon_{i,t} \quad (2)$$

Again, we report specifications with and without a squared aid term. According to Clemens et al. (2012), the appropriate method to test the effect of aid on economic growth accounts for the non-linear effect of aid, removes country fixed-effects through first-differencing, and lags aid by one period. As they argue, this minimizes potential misspecification due to reversed

[20] We exclude the permanent UNSC members from the analysis.

[21] To reduce clutter, we do not show them in all tables. Burnside and Dollar include: Initial GDP/capita, Ethnic Fractionalization, Assassinations, Ethnic Fractionalization*Assassinations, dummies for Sub-Saharan Afric and East Asia, Institutional Quality, M2/GDP (lagged), Policy, and period dummies. Rajan and Subramanian: Initial GDP/capita, Initial Policy, (log) Initial Life Expectancy, Geography, Institutional Quality, (log) Inflation, Initial M2/GDP, Budget Balance/GDP, Revolutions, Ethnic Fractionalization, period dummies and dummies for Sub-Saharan Africa and East Asia. The original studies include time-invariant variables that are removed in (2) below (as in Clemens et al.) through taking differences. Appendix A reports the sources and definitions of all variables, while we show descriptive statistics in Appendix B. Appendix C reports the full specifications for the main regressions.

[22] It could be argued that temporary UNSC membership should be interacted with aid squared as well. Political motivation would then not only change the level of the marginal effect of aid, but also its slope. Such an interaction effect, however, is not significant in our preferred specification (the p-value being 0.82 in the BD sample and 0.22 in the RS sample). Detailed calculations are available on request.

[23] Clemens et al. do not explain why they prefer the first-differences regressions over fixed effects regressions.

causality between aid and growth, and omitted variables bias.[24] This is our preferred estimation strategy.[25]

The regression of Bueno de Mesquita and Smith (2010) is a slightly different one. The dependent variable in Bueno de Mesquita and Smith is again the growth rate of per capita GDP over a four-year-period. However, they compare the difference in growth over these four years for countries that have been a temporary member of the UNSC in the first year of a period to those countries that have not been members in the same period. Rather than including a measure of aid, they estimate the effect of a dummy indicating UNSC membership per se and attribute its effect to foreign aid (or other types of loose money, see Bueno de Mesquita and Smith 2013). We use their baseline specification, and add the UNSC and aid variables, and the interaction of these variables to the equation. The model is thus no longer in first-differences, but instead includes country fixed effects in the main regressions. The lag structure replicates our approach above.[26]

Column 1 of Table I.1 shows the results for the Burnside and Dollar (BD) regressions on the extended data of Clemens et al. (2012), covering the 1970-2005 period. All data are averaged over four years. The dependent variable is the average annual growth rate of real GDP per

[24] In addition, they seem to prefer a measure of early-impact aid over all aid. This measure has been shown not to be a robust predictor of growth (Rajan and Subramanian 2008, Bjørnskov 2013). What is more, a major drawback with this measure is that disaggregated aid disbursements are not available for the entire period, so that disbursements have to be estimated based on commitments. Data on commitments in the earlier periods also suffer from severe underreporting, which is not addressed in Clemens et al. (2012) (see OECD/DAC CRS Guide, Coverage Ratios, accessed on March 3, 2014: http://www.oecd.org/dac/stats/crsguide.htm). We therefore prefer to focus on overall aid. To the extent that parts of aid are not systematically related to growth, the larger noise reduces the probability that we find a significant effect. As outlined above, we lag disbursements by one period to account for timing.

[25] One could argue that UNSC membership should be included in differences instead of levels. To us, it seems intuitive that the level rather than changes in UNSC membership conditions the effectiveness of changes in aid. Nevertheless, when we first-difference UNSC membership, the results are similar. The interaction remains negative and significant at the one-percent level in the BD sample, and significant at the ten-percent level in the RS sample.

[26] We use the share of temporary UNSC membership lagged by two four-year periods, aid disbursements as a percentage of GDP lagged by one period, and their interaction. Consistent with the original setup, the four-year periods in this specification can be understood as moving averages. For example, growth in the 1991-1994 period is related to aid disbursements in the 1987-1990 period.

capita; aid is measured as net Official Development Assistance (ODA) as a percentage of GDP.[27] Column 2 focuses on Clemens et al.'s permutations of Rajan and Subramanian (RS) to test whether our results are due to the specific setup of the BD specifications. The RS regressions use data averaged over five (rather than four) years, and the extended sample of Clemens et al. (2012) covers the 1971-2005 period.[28] Before we turn to testing specification (1) (described above), we use the first two columns to focus on the effect of contemporaneous aid disbursements, conditional on UNSC membership in the previous period, and omit aid squared. While the table reports the variables of interest only, we report the full model for our preferred specifications (columns 7 and 8) in Appendix I.C.

As can be seen in column 1, the interaction between aid and the share of years the recipient has been a temporary member of the UNSC in the previous period is not significant at conventional levels. This is intuitive, as we cannot expect the effect of disbursements on growth to be immediate (Clemens et al. 2012). However, according to column 2 the coefficient is significant at the ten-percent level in the RS sample, suggesting a negative effect of political motivations even for contemporaneous aid. Part of the aid committed in the previous period might already have been disbursed (and affected growth) in that period or the modes of aid delivery – project preparation, conditionality etc. – of previously committed aid might have changed.

Columns 3 and 4 show how the timing of the aid-variable affects the outcome. When we lag aid by one period, we consequently lag the share of years a country is a member on the UNSC by two periods (as shown in equation (1) above but excluding aid squared). As Clemens et al. argue, this should substantially raise the coefficient of aid. While the coefficients of the aid variable are not significant at conventional levels, they do increase in

[27] The original source for GDP per capita growth is the World Bank's World Development Indicators; ODA is total net ODA in current US$ from Table 2 of the OECD's Development Assistance Committee in percent of GDP in current US$, taken from the World Development Indicators (see the Technical Appendix to Clemens et al. 2012).

[28] The data for per capita GDP growth are originally calculated based on the Penn World Tables, updated by Clemens et al. for the year 2005 using the World Development Indicators. Net ODA is measured in the same way as in the BD regressions (see the Technical Appendix to Clemens et al. 2012).

magnitude. The resulting interaction between temporary UNSC membership and aid is negative and significant at the ten-percent level in the BD specification (column 3), but not significant in the model of RS (column 4).

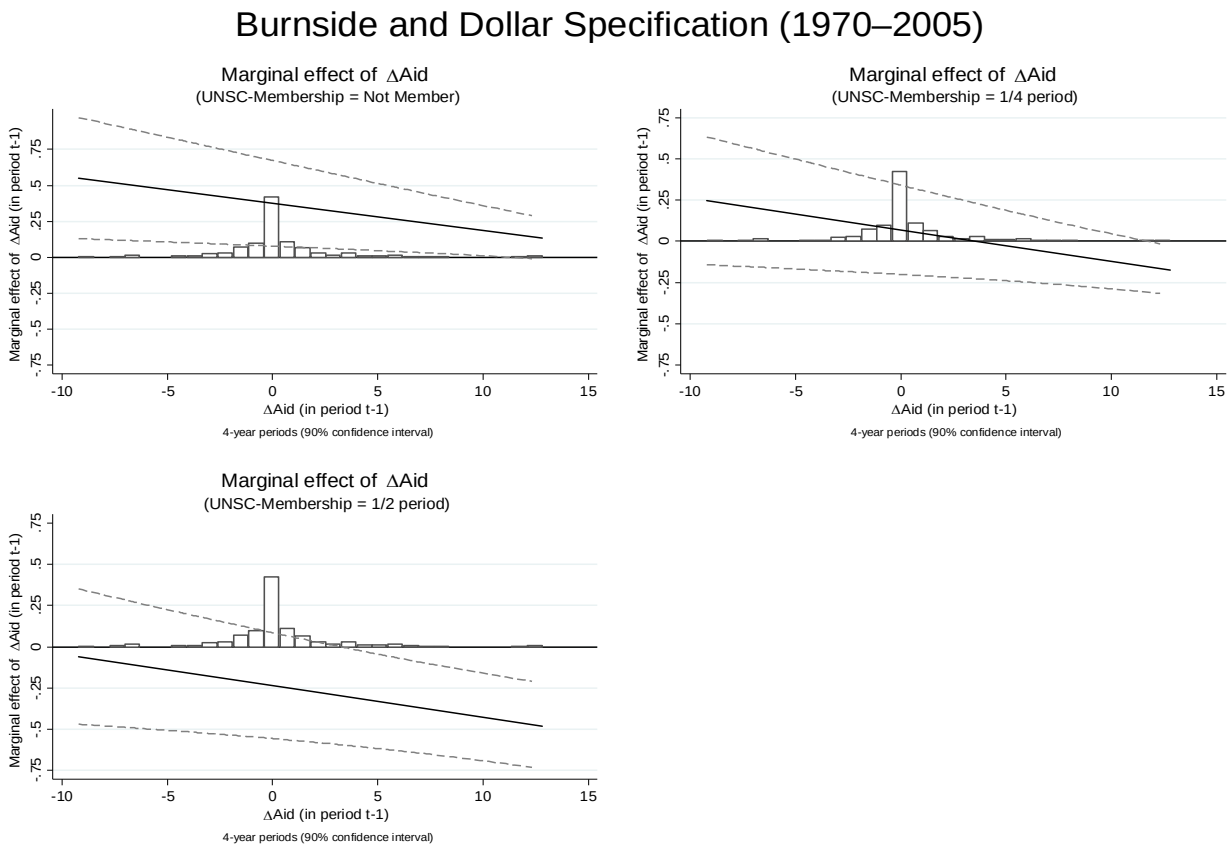
Note that aid by itself is not significant at conventional levels in any of the four specifications. This is in line with the results in Clemens et al. (2012) and clearly does not imply that aid is ineffective. If more aid is given to countries which are in greater need, and if those tend to have lower growth rates, the insignificant coefficients for aid might reflect this relationship rather than the absence of positive effects of aid on growth. If aid and growth are persistent over time, this holds whether or not we use lagged values of aid.

We next turn to our preferred estimations, which first-difference the dependent and the explanatory variables except membership on the UNSC (equation 2). This specification takes account of systematic time-invariant differences between members and non-members of the UNSC and their effect on growth. We report specifications excluding aid squared (columns 5 and 6) and including it (columns 7 and 8), to account for potentially diminishing returns to aid. The results support our hypothesis that aid committed during times of short-term political importance is indeed less effective.[29] When we do not account for diminishing returns to aid by including aid squared, the coefficient of the interaction term is negative and significant at the five-percent level in the BD specification (column 5) and negative and significant at the ten-percent level in the RS specification (column 6). When we include aid

[29] Also note that UNSC membership is negative and significant at the ten-percent level according to columns 5 and 7. This implies that UNSC membership reduces growth even in the absence of any aid. This is broadly in line with Bashir and Lim (2013), who find a direct effect of UNSC membership on growth controlled for aid inflows. The effect is however not robust. If we control for the (first-differenced) set of institutional variables provided by the International Country Risk Guide (ICRG) the coefficient of UNSC is no longer significant at conventional levels, while the coefficient of the interaction term stays significant at the one percent level in our preferred specification (of column 7).

squared,[30] the interaction becomes significant at the one- and five-percent level, respectively (columns 7 and 8).[31]

Figure I.5: Marginal effect of changes in aid disbursements



Notes: Marginal effect of changes in aid disbursements on changes in economic growth conditional on changes in aid disbursements and varying temporary UNSC membership (based on Table I.1, column 7). The histogram shows the distribution of ΔAid in the regression sample. Note that the significant interaction term in the regression shows that these marginal effects differ significantly from each other. The derivation of the marginal effects can be found in Appendix D.

[30] Part of the literature on the effect of aid on growth argues that aid squared has to be included in a meaningful growth regression, e.g., Durbarry et al. (1998). However, see Doucouliagos and Paldam (2009) for a critique.

[31] We also tested whether the effect differs when we take only important years of UNSC membership into account, as suggested in Kuziemko and Werker (2006). The results for the BD specification remain unchanged; in the RS specification the interaction term becomes insignificant, however. This is not surprising given that their measure is based on US newspapers and thus measures the importance of the UNSC predominantly for the United States rather than the average donor.

Figure 5 shows the marginal effects for the BD model and the 90%-confidence intervals.[32] As can be seen, the marginal effect of changes in aid on changes in growth depends on the magnitude of the change in aid and on membership on the UNSC. The effect declines for higher values of ΔAid, reflecting diminishing returns to aid.[33] For any value of ΔAid, the effectiveness of aid decreases with the number of years the recipient country has spent on the UNSC two periods before (i.e., when the aid has been committed). The average effect of a 1 percentage point increase in aid as a percentage of GDP on yearly economic growth is 0.61 percentage points higher if the recipient has not served on the UNSC compared to if it has served two years (i.e., 1/2 of the four-year-period). The aid-growth relationship is positive for countries that have not served on the UNSC when aid has been committed,[34] while being largely insignificant for countries that have served one year, and significantly negative for those that have served two years and for which the increases in aid exceed 3 percent of GDP.

Table I.2 reproduces the regressions in first differences (including aid squared) focusing on Africa only. African nations follow the strictest norm of rotation on the UNSC among all regional election caucuses, making the exogeneity of UNSC membership particularly hard to challenge (Dreher et al. 2014). The results are similar to those for all countries, as shown above. The coefficient on the interaction term is negative and significant at the five-percent level in the Burnside and Dollar regressions. The coefficients in the Rajan and Subramanian specification are, however, no longer significant at conventional levels. This is potentially due to the substantially smaller sample, and the fact that the five-year periods used by RS make it more difficult to capture the correct timing as proposed in our timeline.[35]

---

[32] A similar figure for the RS model is available on request.

[33] The marginal effect of a change in aid is linear in the lagged difference and in the twice-lagged level of aid (see Appendix D).

[34] This holds unless the change in aid exceeds 10 percent of GDP.

[35] As a substantial share of politically motivated aid inflows come from the United States, we replicated the analysis focusing on US aid only. This comes with two potential problems that might bias against finding a significant interaction: First, overall US aid might be politically motivated to a larger extent than ODA from all donors. It could then be difficult to differentiate between the growth-effects of normal aid and aid given during the recipient's time as temporary UNSC member. Second, it might not be possible to detect significant effects when focusing on aid from one donor exclusively as such aid might be insufficiently large to measurably affect growth. Our results are similar to those for all aid, but generally weaker: The interaction terms remain negative in

In Table I.3 we turn to the model of Bueno de Mesquita and Smith (BdM/Smith).[36] Column 1 includes fixed effects for years and regions, but not for countries. As can be seen, countries that were temporary members of the UNSC at the beginning of a four-year-period do not experience significantly different rates of growth.[37] In column 2 we lag temporary membership on the UNSC by two periods. As can be seen, the twice-lagged effect of UNSC membership does not reduce growth at conventional levels of significance, indicating that UNSC membership per se does not hurt growth.

In accordance with our timeline, we again assume that aid which is committed while a country is on the UNSC gets disbursed with a lag of about one four-year-period and affects economic growth in the period following disbursement. Column 3 adds aid lagged by one period, twice-lagged UNSC membership and their interaction to the equation. Column 4 shows the same specification, but restricts the sample to Africa. The results are in line with those above, with the interaction between UNSC membership and aid being negative and significant at the five- and one-percent level respectively.

In columns 5-8 we replace the region-fixed effects with dummies for each country and add regional quartic time trends (as in Bueno de Mesquita and Smith 2010). It is thus the more rigorous specification, as it accounts for potential time-invariant omitted variables, different forms of regional trends, and common yearly shocks. The results are broadly in line with

---

the main regressions, but become significant at the one-percent level only in the BD specification in the Africa sample. Interestingly, however, we find a negative and significant effect at the one and ten-percent level respectively for autocratic countries. This supports the notion that the adverse effects of politically motivated aid are a particular concern in autocratic countries, which might not receive any aid without UNSC membership (see section 3 below).

[36] Their source for GDP per capita growth is the World Bank's World Development Indicators (2007), measured in constant 2000 US$. Aid is measured as net official development assistance in percent of GDP and comprises aid from all sources (also taken from the World Development Indicators 2007). All regressions include as explanatory variables: (log) population size, (log) per capita GDP, the level of democracy and its interaction with UNSC membership (as do the main specifications in Bueno de Mesquita and Smith 2010). Note that contrary to Bueno de Mesquita and Smith we exclude high-income countries (as defined by the World Bank) from the sample, as they do not receive any aid. Again, we restrict the table to the variables of main interest and report the full specification for our preferred model in Appendix C.

[37] This is contrary to the results of Bueno de Mesquita and Smith (2010). Unlike them, we cluster standard errors at the recipient country level. Without clustering, the negative coefficient is significant at the ten-percent level.

those above. While the interaction between aid and membership on the UNSC is negative but not significant at conventional levels for the overall sample (column 7), it is negative and significant at the one-percent level in the regressions focusing on Africa (column 8). As explained above, African countries provide the most reliably exogenous variation in politically motivated aid; thus a causal interpretation of this result is most warranted. Overall, our results support the hypothesis of an adverse effect of political interests on aid effectiveness. That is, politics matter.[38]

The next section extends the main analysis by separately investigating democratic and autocratic recipients of aid and investigating possible transmission channels.

3. Extensions

As Nooruddin and Vreeland (2010) argue, UNSC votes of democratic countries provide greater legitimacy and are thus more valuable than those of non-democratic ones. Democracies should consequently have particular leverage while serving on the UNSC, potentially reducing the effectiveness of aid more strongly than aid given to autocracies. On the other hand, Bueno de Mesquita and Smith (2010) report the adverse effects of UNSC membership to be stronger in autocracies. This is because, on average, the potential to misuse aid is higher in autocracies. On balance, we expect a more pronounced interaction effect in autocracies. In the next set of regressions we investigate the effect of politically motivated aid in democracies and autocracies separately, measured according to the indicator of Cheibub et al. (2010).

[38] We also run separate regressions for the period of the Cold War and the post-Cold War period. As Berthélemy and Tichit (2004) show, the importance of colonial ties diminishes with the end of the Cold War. Headey (2008) also shows that bilateral aid became more effective after the end of the Cold War, in line with Dunning's (2004) analysis of how aid affected the spread of democracy. If donors gained greater leverage to enforce conditions after the end of the Cold War, and the accompanying risk of losing an ally to the opposing bloc decreased, we would expect the effect of geopolitical aid to be particularly harmful during the Cold War era. Indeed, the negative coefficient of the interaction term is larger during the Cold War era for the BD and RS sample; for the BdM/Smith sample there are no obvious differences visible. We also tested whether politically motivated aid is particularly harmful in times of economic crises, as suggested in Dreher et al. (2013). We find no systematic difference.

Table I.4 reports the results for the BD and RS specifications, while Table I.5 shows them according to the specification of BdM/Smith. In Table I.4 we focus on those regressions that control for time-invariant omitted variables by first-differencing the equation. For the BD sample the negative interaction is significant at the one-percent level in autocracies (columns 3 and 7) and larger than in democracies, where it is significant at the ten-percent level when aid squared is included (column 5) and insignificant without aid squared (column 1). In both models, the negative coefficients are substantially larger in magnitude in autocracies than in democracies. The RS specifications show positive coefficients for democracies and negative coefficients for autocracies, all failing to reach statistical significance however.

Table I.5 shows a similar picture for the BdM/Smith specification, where only the interactions in autocracies have a negative coefficient. The negative effect is significant when we control for regional and time fixed effects (column 3). When we add time trends and country fixed effects in column 4 the coefficient remains negative but turns insignificant. In democracies the interaction turns *positive* and significant at the five-percent level with region fixed effects (column 1), and insignificant with country fixed effects (column 2). Overall, the greater political legitimacy of democratic countries' votes on the UNSC does not seem to drive the results. Our results suggest that in autocratic countries which have potentially less interest in promoting development, the reduced pressure to use development aid for developmental purposes during UNSC membership might be particularly harmful. Given that autocracies are, on average, countries where the potential role of the donor in pushing for change is most prevalent, the adverse consequences of politically motivated aid are particularly unfortunate.

The results so far support our proposed timeline. However, this does not preclude other timings to be potentially important. Thus, Table I.6 reports results from regressions that examine whether and to what extent other possible timings are supported by the data.

To test these possibilities, we replicate the regressions of Table I.1, columns 7 and 8, for the BD and RS specifications respectively using alternative timelines. For BdM/Smith we focus on the specification of column 7 in Table I.3. We test if the effectiveness of aid disbursed in different periods is affected by UNSC membership in the same period, one period before, and two periods before. For example, if aid disbursed during UNSC membership is less

effective if the country has been on the UNSC in the previous period, contemporaneous membership could affect compliance via conditionality.

While Table I.6 shows the coefficients and standard errors of the interaction terms only, note that the respective aid, aid squared and UNSC variables are also included in each regression (as are the remaining control variables). We also report the coefficients following our previously proposed and theoretically most likely timeline (Aid_{t-1}*$UNSC_{t-2}$) for comparison. As can be seen, all other interactions are not significant at conventional levels, except the specification following BdM/Smith (column 3) for Aid_{t-1}*$UNSC_{t-1}$. The table shows that the interaction is significant at the one-percent level, with a negative coefficient. This result implies that part of the aid committed during membership gets disbursed in the same period and is thus less effective one period later. Overall, and in particular for the BD and RS specifications that employ a more rigorous set of control variables than BdM/Smith, the regressions support our proposed timeline, and thus the theoretical considerations underlying it.

What can explain these results? The previous literature identified a number of transmission channels for individual donors. Dreher et al. (2013) showed that political motives reduce the quality of World Bank projects. Also for the World Bank, Kilby (2011) reported that political allies are allowed to start projects with inferior preparation. Stone (2008) found that political favoritism undermines the credibility of IMF conditionality.

In order to test these transmission channels in our broader sample of aid by all DAC donors, we would require data on aid conditionality and compliance with these conditions, project success, and time and resources invested in project preparation. These data do not exist for a broad sample of donors. Data exist, however, on different aid modalities and the sectoral composition of aid across recipient countries on and off the UNSC. Previous research argues that the effectiveness of aid depends on the sector the aid is given to and the modalities

through which it is delivered (Bjørnskov 2013). To the extent that UNSC membership affects composition and modalities,[39] the effectiveness of aid would change.

While a detailed analysis is beyond the scope of this paper, Table I.7 reports the amount of aid committed to the individual sectors while countries have been temporary members of the UNSC and at other times (in constant million 2011 US$). As can be seen, there are substantial differences between countries on and off the UNSC. Table I.7 also reports a t-test for equality of a certain category's share in total aid committed to UNSC members and non-members. The results show that the share increases significantly in 7 of the 26 sectors, and decreases in one sector. For example, UNSC members receive larger general budget support (+46%), more aid for other social infrastructure (+105%), larger food aid (+59%), and less emergency aid (-39%). According to Nunn and Qian (2013), US food aid increases the risk of civil conflict. Bjørnskov (2013) shows that a category of aid that includes emergency aid increases growth. Both increases in food aid and reductions in emergency aid are thus likely to reduce the effectiveness of aid.

Strong differences also arise when we focus on the type of aid, as we show in Table I.8. The results indicate increases in all types of aid for temporary members of the UNSC. In particular, budget aid increases by 192% during UNSC membership, while the increase in project aid is 95%. Loans increase by 137% and grants by 32%. The increases of these types of aid in a recipient's overall aid are all statistically significant at the one-percent level. Note that budget support is the type of aid that offers most flexibility to the recipient government and is thus particularly attractive to use for political reasons. To the extent that these different types of aid affect economic growth differently, the different composition of aid might also explain the effect that we identified in this paper.

While we leave further explorations of the exact channels that explain the lower effectiveness of aid given to countries of short-term geopolitical importance for future research, these

[39] Bayer et al. (2014) provide initial evidence. Their results show that countries prefer to work with UN agencies rather than the World Bank in implementing projects under the Global Environment Facility while being on the UNSC.

descriptive statistics show striking differences in how certain types of aid and aid to specific sectors change as a consequence of a country's changing political importance.

4. Conclusion

In this paper we addressed the question of whether a recipient's short-term geopolitical importance reduces the effectiveness of its aid receipts. We made use of a straightforward proxy for the geopolitical importance of a country. Specifically, we exploited the quasi-random variation in aid commitments and modalities of delivery resulting from the recipient being of extraordinary geopolitical importance during its temporary membership on the UNSC. The previous literature has shown that temporary members of the UNSC receive substantial and unusual increases in aid (Kuziemko and Werker 2006, Dreher et al. 2009a, 2009b). To the extent that political motives for the allocation of aid affect its consequences, the aid a country receives while serving on the UNSC should be less effective on average. The literature also found that the time spent to prepare aid projects, the number of aid conditions as well as punishment of non-compliance with such conditions differ for politically important aid recipients. Overall, we therefore expect aid given to countries of short-term political importance to be less effective in promoting growth than aid given at other times.

Rather than suggesting our own econometric model, we augmented three widely cited specifications from the literature (Burnside and Dollar 2000, Rajan and Subramanian 2008, Bueno de Mesquita and Smith 2010) with our exogenous measure (e.g., Bueno de Mesquita and Smith 2010, Vreeland and Dreher 2014) of politically motivated aid. Our results show that aid committed while a recipient has been a member of the UNSC is less effective in terms of increasing economic growth. This holds in particular in autocratically governed recipient countries. It also holds when we restrict our sample to African countries, which follow the strictest norm of rotation on the UNSC. That is, foreign aid committed to countries of short-term geopolitical importance is less effective than aid committed at other times particularly in those places where development would be most needed.

While we did not aim to rigorously test whether aid is effective, but rather, whether aid effectiveness is reduced due to the short-term political importance of recipients, our findings have direct implications for the existing and future aid effectiveness literature. To the extent the reader accepts the regressions presented in Clemens et al. (2012) and Bueno de Mesquita and Smith (2010) as causal tests for the effectiveness of aid, our results imply that overall aid tends to increase growth, while aid given to countries of geopolitical importance is insignificant, or even harmful to growth. In any case, aid to important countries is less effective than aid given at other times. Political motives channel more aid to temporary UNSC members whose subsequent growth rates might increase to the extent that the marginal effect of aid remains positive. This increase would however come at the cost of reduced aid and larger losses of growth elsewhere.

An important implication of our results relates to the identification strategy in the previous aid effectiveness literature, much of which tries to identify the causal effects of overall aid by instrumenting for aid using political variables. Our results show that geopolitical variables are invalid as instruments for overall aid when "political aid" is different.[40] The results of previous studies aiming to identify the effect of all aid on growth by relying on variation caused by changing political alliances thus have to be treated with caution. More specifically, our results show that the estimated effects of politically motivated aid – mistakenly reported as effect of all aid in these studies – represent the lower bound of the true effect of all aid.

In terms of increasing the effectiveness of aid, there are arguably two possibilities. First, foreign aid could be separated from political motives, so that it truly becomes "development aid." Given the incentives of donors to use aid to achieve their geopolitical goals this is unlikely to happen. Second, the exact channels by which geopolitical motives reduce the effectiveness of aid should be identified. The choice of a suitable remedy would depend upon which of the channels outlined above is responsible for the reduced effectiveness of aid. We leave such analysis for future research.

[40] See also Fleck and Kilby (2006), Headey (2008), Bearce and Tirone (2010), Minoiu and Reddy (2010), Kilby and Dreher (2010), and Faye and Niehaus (2012).

5. Tables and figures

Table I.1: Politically motivated aid and growth, OLS, BD and RS

	Burnside and Dollar		Rajan and Subramanian	
	(1)		(2)	
	Coef.	Std. err.	Coef.	Std. err.
Aid (t)	0.010	(0.033)	-0.004	(0.040)
UNSC (t-1)	1.171	(0.888)	0.854	(1.283)
UNSC (t-1)*Aid (t)	-0.045	(0.116)	-0.361*	(0.203)
First difference?	No		No	
Adj. R-Squared	0.30		0.32	
Number of Observations	418		432	
Number of Countries	54		67	
	(3)		(4)	
Aid (t-1)	0.056	(0.045)	0.005	(0.056)
UNSC (t-2)	0.255	(0.973)	-0.501	(1.058)
UNSC (t-2)*Aid (t-1)	-0.329*	(0.166)	0.010	(0.149)
First difference?	No		No	
Adj. R-Squared	0.31		0.30	
Number of Observations	418		432	
Number of Countries	54		67	
	(5)		(6)	
Aid (t-1)	0.121	(0.095)	0.149*	(0.085)
UNSC (t-2)	-1.679*	(0.903)	-0.866	(1.420)
UNSC (t-2)*Aid (t-1)	-0.927**	(0.429)	-1.094*	(0.590)
First difference?	Yes		Yes	
Adj. R-Squared	0.18		0.30	
Number of Observations	361		351	
Number of Countries	54		64	
	(7)		(8)	
Aid (t-1)	0.453**	(0.189)	0.356**	(0.148)
Aid (t-1) squared	-0.010**	(0.004)	-0.007	(0.004)
UNSC (t-2)	-1.649*	(0.992)	-0.947	(1.402)
UNSC (t-2)*Aid (t-1)	-1.222***	(0.369)	-1.365**	(0.647)
First difference?	Yes		Yes	
Adj. R-Squared	0.29		0.31	
Number of Observations	361		351	
Number of Countries	54		64	

Notes: The dependent variable is growth of real GDP per capita. All "Burnside and Dollar" regressions use averages over four years and include Initial GDP/capita, Ethnic Fractionalization, Assassinations, Ethnic Fractionalization*Assassinations, dummies for Sub-Saharan Africa and East Asia, Institutional Quality, M2/GDP (lagged), Policy, and period dummies. The dependent variable covers the 1970-2005 period (corresponding to Clemens et al. 2012, Table I7, columns 1 and 7). All "Rajan and Subramanian" regressions use averages over five years and include Initial GDP/capita, Initial Policy, (log) Initial Life Expectancy, Geography, Institutional Quality, (log) Inflation, Initial M2/GDP, Budget Balance/GDP, Revolutions, Ethnic Fractionalization, period dummies and dummies for Sub-Saharan Africa and East Asia. The dependent variable covers the 1966-2005 period (using the full extended sample provided by Clemens et al. 2012, Table I.9). Standard errors in parentheses (clustered at the recipient country level). * p<0.10, ** p<0.05, *** p<0.01.

Table I.2: Politically motivated aid and growth in Africa, OLS, BD and RS

	Burnside and Dollar		Rajan and Subramanian	
	(1)		(2)	
	Coef.	Std. err.	Coef.	Std. err.
Aid (t-1)	0.138	(0.105)	0.026	(0.126)
UNSC (t-2)	-1.243	(1.760)	-1.506	(3.905)
UNSC (t-2)*Aid (t-1)	-1.448**	(0.650)	0.092	(1.425)
First difference?	Yes		Yes	
Adj. R-Squared	0.15		0.31	
Number of Observations	103		94	
Number of Countries	20		20	
	(3)		(4)	
Aid (t-1)	0.239	(0.178)	0.247	(0.291)
Aid (t-1) squared	-0.002	(0.003)	-0.006	(0.005)
UNSC (t-2)	-1.242	(1.801)	-1.411	(3.937)
UNSC (t-2)*Aid (t-1)	-1.480**	(0.666)	-0.333	(1.527)
First difference?	Yes		Yes	
Adj. R-Squared	0.15		0.31	
Number of Observations	103		94	
Number of Countries	20		20	

Notes: The dependent variable is growth of real GDP per capita. All "Burnside and Dollar" regressions use averages over four years and include Initial GDP/capita, Ethnic Fractionalization, Assassinations, Ethnic Fractionalization*Assassinations, a dummy for Sub-Saharan Africa, Institutional Quality, M2/GDP (lagged), Policy, and period dummies. The dependent variable covers the 1970-2005 period (corresponding to Clemens et al. 2012, Table I.7, columns 1 and 7). All "Rajan and Subramanian" regressions use averages over five years and include Initial GDP/capita, Initial Policy, (log) Initial Life Expectancy, Geography, Institutional Quality, (log) Inflation, Initial M2/GDP, Budget Balance/GDP, Revolutions, Ethnic Fractionalization, period dummies and a dummy for Sub-Saharan Africa. The dependent variable covers the 1966-2005 period (using the full extended sample provided by Clemens et al. 2012, Table I.9). Standard errors in parentheses (clustered at the recipient country level). ** $p<0.05$.

Table I.3: Politically motivated aid and growth, OLS, BdM/Smith

	(1)		(2)		(3)		(4)	
	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.
UNSC (t)	-1.203	(1.316)						
UNSC (t-2)			-1.611	(1.287)	-0.307	(1.401)	3.420*	(1.979)
Aid (t-1)					0.493***	(0.159)	0.440**	(0.191)
UNSC (t-2)*Aid (t-1)					-0.199**	(0.097)	-0.381***	(0.120)
Country Fixed Effects	No		No		No		No	
Region Fixed Effects	Yes		Yes		Yes		Yes	
Year Dummies	Yes		Yes		Yes		Yes	
Regional Trend Variables	No		No		No		No	
Adj. R-Squared	0.26		0.26		0.17		0.13	
	(5)		(6)		(7)		(8)	
UNSC (t)	-0.523	(0.999)						
UNSC (t-2)			-0.763	(1.180)	-0.93	(1.299)	2.774*	(1.568)
Aid (t-1)					0.273***	(0.103)	0.247	(0.170)
UNSC (t-2)*Aid (t-1)					-0.024	(0.077)	-0.175***	(0.060)
Country Fixed Effects	Yes		Yes		Yes		Yes	
Region Fixed Effects	No		No		No		No	
Year Dummies	Yes		Yes		Yes		Yes	
Regional Trend Variables	Yes		Yes		Yes		Yes	
Adj. R-Squared	0.43		0.43		0.45		0.41	
Sample	all		all		all		Africa	
Number of Observations	3516		3516		3378		1272	
Number of Countries	119		119		119		42	

Notes: The dependent variable is growth of real GDP per capita for the 1960-2005 period. All regressions include (log) Population Size, (log) GDP per capita, the level of Democracy and its interaction with UNSC Membership. The sample is based on the data made available by BDM/Smith, Table I.4. Standard errors in parentheses (clustered at the recipient country level). * p<0.10, ** p<0.05, *** p<0.01.

Table I.4: Politically motivated aid and growth, OLS, BD and RS, by regime type

	Democracy				Autocracy			
	Burnside and Dollar		Rajan and Subramanian		Burnside and Dollar		Rajan and Subramanian	
	(1)		(2)		(3)		(4)	
	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.
Aid (t-1)	0.071	(0.115)	0.247*	(0.137)	0.171	(0.132)	0.082	(0.106)
UNSC (t-2)	-0.014	(0.994)	0.381	(1.500)	-2.315	(1.474)	-1.119	(2.062)
UNSC (t-2)*Aid (t-1)	-0.236	(0.412)	0.712	(1.235)	-1.106***	(0.328)	-0.781	(0.602)
First difference?	Yes		Yes		Yes		Yes	
Adj. R-Squared	0.31		0.35		0.14		0.26	
Number of Observations	122		115		195		230	
Number of Countries	28		33		45		53	
	(5)		(6)		(7)		(8)	
Aid (t-1)	0.521**	(0.208)	0.498*	(0.265)	0.440*	(0.249)	0.238	(0.170)
Aid (t-1) squared	-0.010**	(0.004)	-0.007	(0.005)	-0.009*	(0.005)	-0.005	(0.005)
UNSC (t-2)	-0.053	(0.994)	0.225	(1.500)	-2.249	(1.485)	-1.183	(2.046)
UNSC (t-2)*Aid (t-1)	-0.825*	(0.460)	0.174	(1.385)	-1.230***	(0.323)	-0.973	(0.649)
First difference?	Yes		Yes		Yes		Yes	
Adj. R-Squared	0.32		0.36		0.15		0.26	
Number of Observations	134		115		220		230	
Number of Countries	28		33		45		53	

Notes: The dependent variable is growth of real GDP per capita. All "Burnside and Dollar" regressions use averages over four years and include Initial GDP/capita, Ethnic Fractionalization, Assassinations, Ethnic Fractionalization*Assassinations, dummies for Sub-Saharan Africa and East Asia, Institutional Quality, M2/GDP (lagged), Policy, and period dummies. The dependent variable covers the 1970-2005 period (corresponding to Clemens et al. 2012, Table I.7, columns 1 and 7). All "Rajan and Subramanian" regressions use averages over five years and include Initial GDP/capita, Initial Policy, (log) Initial Life Expectancy, Geography, Institutional Quality, (log) Inflation, Initial M2/GDP, Budget Balance/GDP, Revolutions, Ethnic Fractionalization, period dummies and dummies for Sub-Saharan Africa. The dependent variable covers the 1966-2005 period (using the full extended sample provided by Clemens et al. 2012, Table I.9). Standard errors in parentheses (clustered at the recipient country level). * p<0.10, ** p<0.05, *** p<0.01.

Table I.5: Politically motivated aid and growth, OLS, BdM/Smith, by regime type

	Democracy				Autocracy			
	(1)		(2)		(3)		(4)	
	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.
UNSC (t-2)	-2.518***	(0.810)	-2.307**	(0.973)	-0.057	(1.644)	0.009	(1.387)
Aid (t-1)	0.018	(0.056)	-0.034	(0.096)	0.359	(0.271)	0.288	(0.334)
UNSC (t-2)*Aid (t-1)	0.257**	(0.122)	0.064	(0.145)	-0.237*	(0.126)	-0.100	(0.093)
Country Fixed Effects	No		Yes		No		Yes	
Region Fixed Effects	Yes		No		Yes		No	
Year Dummies	Yes		Yes		Yes		Yes	
Regional Trend Variables	No		Yes		No		Yes	
Adj. R-Squared	0.50		0.75		0.17		0.50	
Number of Observations	889		889		2295		2295	
Number of Countries	75		75		102		102	

Notes: The dependent variable is growth of real GDP per capita for the 1960-2005 period. All regressions include (log) Population Size, (log) per capita GDP, the level of democracy and its interaction with temporary UNSC Membership. The sample is based on the data made available by BDM/Smith, Table I.4. Standard errors in parentheses (clustered at the recipient country level). * p<0.10, ** p<0.05, *** p<0.01.

Table I.6: Politically motivated aid and growth, different timelines

	Burnside-Dollar		Rajan-Subramanian		Bueno de Mesquita-Smith	
	(1)		(2)		(3)	
	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.
Aid (t) *UNSC (t)	-0.432	(0.471)	0.074	(0.333)	0.050	(0.076)
Aid (t) *UNSC (t-1)	0.272	(0.475)	0.014	(0.507)	-0.042	(0.115)
Aid (t) *UNSC (t-2)	0.200	(0.165)	-0.217	(0.506)	0.021	(0.101)
Aid (t-1) *UNSC (t-1)	0.038	(0.418)	0.479	(0.482)	-0.196***	(0.058)
Aid (t-1) *UNSC (t-2)	-1.222***	(0.364)	-1.365**	(0.647)	-0.024	(0.077)
Aid (t-2) *UNSC (t-2)	-0.029	(0.469)	-0.079	(0.105)	-0.048	(0.090)

Notes: The dependent variable is growth of real GDP per capita. Columns 1 and 2 are based on Table I.1, columns 7 and 8, respectively. Column 3 is based on column 7 in Table I.3. All "Burnside-Dollar" regressions are in first differences, use averages over four years, and include Initial GDP/capita, Ethnic Fractionalization, Assassinations, Ethnic Fractionalization*Assassinations, dummies for Sub-Saharan Africa and East Asia, Institutional Quality, M2/GDP (lagged), Policy, aid squared and period dummies. The "Rajan-Subramanian" regressions are in first differences, use averages over five years, and include Initial GDP/capita, Initial Policy, (log) Initial Life Expectancy, Geography, Institutional Quality, (log) Inflation, Initial M2/GDP, Budget Balance/GDP, Revolutions, Ethnic Fractionalization, aid squared, period dummies and dummies for Sub-Saharan Africa and East Asia. "Bueno de Mesquita-Smith" includes (log) Population Size, (log) per capita GDP, the level of Democracy and its interaction with UNSC Membership, country fixed effects, year dummies and regional trend variables. All sources and periods covered correspond to the tables above; variation in samples arises from differences in the lag-structures. Standard errors in parentheses (clustered at the recipient country level). ** $p<0.05$, *** $p<0.01$.

Table I.7: Aid and temporary UNSC Membership according to sectors

Sectoral allocation of total aid committed, 1973-2011, constant million 2011 US$

	Mean			t-test
Sector	Non-UNSC member	UNSC member	Increase in %	p-value
Education	37.62	56.36	50%	0.74
Health	27.48	34.70	26%	0.07
Population	25.79	40.38	57%	0.85
Water and Sanitation	36.62	68.99	88%	0.10
Government /Civil Society	47.78	56.19	18%	0.01
Other Social Infrastructure	18.44	37.74	105%	0.01
Transport and Storage	62.16	93.57	51%	0.84
Communication	10.38	19.70	90%	0.96
Energy Generation and Supply	53.87	100.60	87%	0.73
Banking and Financial Services	13.30	16.93	27%	0.33
Business and other Services	10.14	11.15	10%	0.40
Agriculture and Fishing	53.73	138.60	158%	0.56
Industry/Mining	26.75	69.36	159%	0.22
Trade/Tourism	4.85	5.33	10%	0.58
Environment	14.48	37.49	159%	0.05
Other Multisector	32.96	45.04	37%	0.04
General Budget support	81.13	118.40	46%	0.04
Food Aid	29.10	46.36	59%	0.01
Other Commodity Assistance	33.78	64.37	91%	0.89
Debt	78.08	110.00	41%	0.72
Emergency Reponse	27.50	16.86	-39%	0.00
Reconstruction Relief	14.47	11.37	-21%	0.25
Disaster Prevention	3.26	1.95	-40%	0.24
Admin of Donors	1.73	2.12	23%	0.59
Refugees	3.33	1.92	-42%	0.22
Unspecified	7.03	12.79	82%	0.41

Notes: Differences in aid commitments by aid type for temporary UNSC and non-UNSC members. The t-value indicates significance of the difference between the shares of the respective aid type for UNSC and non-UNSC members. Data source: OECD DAC Creditor Reporting System (CRS) aid activities database.

Table I.8: Aid and UNSC Membership according to type of aid

Allocation of total aid committed, 1973-2011, constant million 2011 US$

	Mean			t-test
Type of Aid	Non-UNSC member	UNSC member	Increase in %	p-value
Budget Aid	69.71	203.60	192%	0.00
Project Aid	240.20	469.40	95%	0.00
Tied Aid	66.44	121.20	82%	0.00
Partially tied Aid	85.40	181	112%	0.00
Untied Aid	275.10	489.50	78%	0.00
Loans	229.60	545.10	137%	0.00
Grants	268.80	354.60	32%	0.03

Notes: Differences in aid commitments by aid type for UNSC members and non- members. The t-value indicates significance of the difference between the shares of the respective aid type for UNSC members and non-members. Data source: OECD DAC Creditor Reporting System (CRS) aid activities database.

II Is there a Home Bias in Sovereign Ratings?

Coauthor:
Andreas Fuchs (Heidelberg University)

Abstract: Credit rating agencies are frequently criticized for producing biased sovereign ratings. This article discusses how the home country of rating agencies could affect rating decisions as a result of political economy influences and cultural distance. Using data from nine agencies based in six countries, we test whether agencies assign better ratings to their home countries, as well as to countries economically, geopolitically and culturally aligned with them. Our results support the existence of biases in favor of the respective home country, culturally more similar countries, and countries in which home-country banks have a larger risk exposure. Linguistic similarity seems to be the main transmission channel for the advantage of the home country.

JEL classification: G24, F34, H63, F65, G15

Keywords: Sovereign debt ratings, Credit rating agencies, Home bias, International finance, Cultural distance, Bank exposure

Acknowledgements: We thank Christian Bjørnskov, Franscesco Caselli, Christian Conrad, Till Cordes, Jonas Dovern, Axel Dreher, Peter Egger, Stephan Klasen, Lukas Menkhoff, Pablo Pinto, Constantin Schön, Rachel Wellhausen, Kevin L. Young, Pedro Forquesato, seminar participants at Heidelberg University (December 2012, May 2013, November 2013), ETH Zurich (September 2013), University of Hannover (December 2013), the Helmut Schmidt University Hamburg (March 2014) and Harvard University (October 2014), as well as conference participants at the Annual Conference of the Canadian Economics Association in Montreal (June 2013), the Annual Meeting of the American Political Science Association in Chicago (August 2013), the European Public Choice Society in Cambridge (April 2014), the Spring Meeting of Young Economists in Vienna (April 2014), the World Congress of the International Economic Association in Jordan (June 2014), the Beyond Basic Questions Workshop in Heidelberg (June 2014), the China Meeting of the Econometric Society in Xiamen (June 2014), and the Annual Conference of the German Economic Association in Hamburg (September 2014) for helpful comments on earlier versions of this paper. Andreas is grateful for support from the Niehaus Center of Globalization and Governance at Princeton University, where part of this research was carried out. Excellent research assistance was provided by Lutz Brückner, Elisabeth Dorfmeister, Sven Kunze and Helge Zille. We further thank Jamie Parsons for proof-reading of earlier versions of this article.

"The assessments by Standard & Poor's appear dictated more by newspaper articles than reality and appear to be tainted by political considerations." (Italy's Prime Minister Silvio Berlusconi after his country was downgraded to "A" in September 2011)

"If I ever dictated anything, it must have been what S&P had to say about domestic Italian economic policy." (Italy's Prime Minister Mario Monti after his country was downgraded to "BBB" in January 2012)

1. Introduction

The accuracy of sovereign ratings is important as rating outcomes can affect the borrowing costs of states (e.g., Afonso et al. 2012), can set de-facto ceilings to corporate ratings (e.g., Durbin and Ng 2005; Borensztein et al. 2013) and – as argued by some – might even trigger or aggravate financial crises (e.g., Ferri et al. 1999; Kaminsky and Schmukler 2002; Gärtner et al. 2011). Ideally, competition and concerns over reputation should incentivize agencies to publish accurate and unbiased ratings. However, some scholars and policymakers around the world accuse credit rating agencies of having unreliable practices, intentionally publishing ratings at unfortunate times and making calculated misjudgments. Germany's finance minister Wolfgang Schäuble speaks of "abusive behavio[u]r," Turkey's president Recep Tayyip Erdoğan makes claims of "unfair" decisions, and José Manuel Barroso, then president of the European Commission, directly accuses the agencies of a "bias […] when it comes to the evaluation of specific issues of Europe."[41] A common argument behind these accusations of biased ratings is that rating outcomes are influenced by factors other than the economic and political fundamentals of rated countries. However, so far, these accusations and complaints have been largely based on individual perceptions or anecdotal evidence.

[41] See media reports by *The Express Tribune* (http://tribune.com.pk/story/211912/breaking-the-oligopoly-ratings-agencies-under-attack-amid-debt-crisis/; accessed 13 June 2013), *Today's Zaman* (http://www.todayszaman.com/news-280044-.html; accessed 13 June 2013), and *BBC News* (http://www.bbc.co.uk/news/business-14043293; accessed 12 September 2013).

Our study provides the first systematic empirical assessment of such claims and focuses on the concerns that revolve around the role of the credit rating agency's "home country."[42] We define "home country" as the country where the agency's headquarters is located or, alternatively, as the country of origin of its major shareholders. Accordingly, we refer to a "home bias" in sovereign ratings if a rating agency assigns a better rating to its home country or to countries with close economic, political and cultural ties to it. As such, a home bias is a deviation of the actual rating level from what would be predicted based on the sovereign's economic and political fundamentals.

Our line of reasoning builds on and adds to the literature on a home bias in investment decisions, bank lending behavior, and trade (e.g., French and Poterba 1991; McCallum 1995; Tesar and Werner 1995; Grinblatt and Keloharju 2001; Flandreau 2006; Presbitero et al. 2014). Several studies have directly or indirectly addressed the question of a home bias in corporate ratings – with mixed results (e.g., Ammer and Packer 2000; Nickell et al. 2000; Shin and Moore 2003; Güttler and Wahrenburg 2007). However, despite the important role they play in determining many countries' access to capital markets, to date there has been no systematic study of home biases in sovereign ratings.

A simple comparison of the sovereign ratings issued by the China-based and Chinese-owned agency Dagong and the big three U.S.-based agencies (Fitch, Moody's and Standard & Poor's) serves as an illustrative example of why one might perceive ratings as influenced by the agency's home country. Compared to the three big U.S-based agencies, Dagong consistently assigns better ratings to the Chinese territories Hong Kong and Macao as well as to the group of BRIC countries, including China itself, while assigning lower ratings to many Western economies.[43] Beyond the case of Dagong, Gaillard (2012: 89) similarly observes that Japanese agencies seem to be "more

[42] According to the EU Justice Commissioner Viviane Reding, for example, "whenever the budgetary situation in the US worsens, certain risk rating agencies turn their guns on Europe" (see http://en.mercopress.com/2012/07/26/ec-criticizes-us-risk-rating-agencies-handling-of-the-euro-zone-crisis; accessed 23 May 2014). Sean Egan, president of the small U.S.-based agency Egan-Jones, criticizes the "current system" as being "New York-centered" and wants to "bring the perspective of China and Russia to the table" with the foundation of the Universal Credit Rating Group (UCRG), a joint venture of agencies from China, Russia and the U.S. (see http://rt.com/business/chinese-ratings-agency-alternative-us-004/, accessed on June 13, 2013).

[43] Note that Dagong and S&P provide both an "AAA" rating to Hong Kong. Comparison as of June 28, 2013. Data from Bloomberg and Fitch.

indulgent, particularly when rating Asian governments, in ways that do not reflect […] official methodologies." The existing literature does not explain these stark differences between agencies. Up until now, scholarship has largely explained sovereign ratings by the economic and political characteristics of rated countries (e.g., Cantor and Packer 1996; Afonso 2003; Archer et al. 2007; Hill et al. 2010). However, systematic research on the role of the home country of credit rating agencies in rating decisions is still lacking – despite the increased attention economists, policy makers and regulators have devoted to the topic since the Global Financial Crisis. Our article fills this gap in the literature.

Why would a rating agency's home country matter for sovereign ratings? We identify steps in the rating process where political economy influences and culture could theoretically affect rating decisions. Specifically, the rating process might be subject to political pressure, the "lobbying" activities of private actors and the self-interests of agency staff. Moreover, cultural distance between the home country of the agency and the rated country could affect ratings. The economic situation of a country that is culturally closer to the home country of the rating agency, for example, might appear more positive to its analysts than a culturally more distant but otherwise comparable country. Thus, our work also contributes to the literature on the effect of cultural biases (e.g., Grinblatt and Keloharju 2001; Guiso et al. 2006, 2009; Giannetti and Yafeh 2012).

This article tests whether, conditional on the economic and political fundamentals of rated countries, credit rating agencies assign better ratings to their home country as well as to countries that are geopolitically and economically aligned with, or culturally similar to it. For this purpose, we use monthly dyadic panel data on sovereign ratings issued by nine rating agencies based in six countries between January 1990 and June 2013. The data have been obtained directly from the rating agencies or accessed and gathered via Bloomberg and cover up to 143 sovereigns. Specifically,

We analyze nine agencies, rather than only the three large U.S.-based ones, for several reasons. First, since all of the agencies are registered in the European Union, Japan and/or the United States, companies and banks can use their ratings to fulfill regulatory requirements. Second, due to the assumed home bias of the US agencies, there is a heated and ongoing debate over the need to

create new agencies or support existing alternatives, particularly in Europe and emerging countries.[44] A thorough examination of the behavior of agencies outside the U.S. sheds light on the meaningfulness and potential benefits of these endeavors. Third, the analysis shows whether potential biases occur only in US agencies, or if a home bias is a general phenomenon. This has policy implications for future regulation, in particular whether and how to curb market concentration and promote competition by supporting smaller agencies.

Our results show empirical evidence that home biases in sovereign ratings exist. First, we find that the average agency assigns a rating to its home country that is almost one point higher than justified by how it assesses other sovereigns. Second, while there is no robust empirical support that geopolitical ties between home and sovereign play a significant role in rating outcomes, agencies provide relatively better ratings to countries in which home-country banks have a larger risk exposure. Third, cultural proximity is related to better ratings: the larger the linguistic differences between home and sovereign, i.e., the more unfamiliar their respective languages are, the lower the assigned rating. The size of the biases increases in the period after the onset of the Global Financial Crisis and varies across agencies. These biases are economically significant and could thus affect borrowing costs and access to international financial markets. The main channel that drives the differential treatment of the home country itself seems to be cultural proximity.

While our analysis is limited to the existing nine global rating agencies that provide sovereign ratings, the results are remarkably robust to the choice of the time period under analysis, the selection of control variables and the estimation methods. We take several steps to come as close as possible to a causal interpretation of our results. First, instead of selecting control variables in an ad-hoc manner, we use a conservative specification that combines the sets of control variables from previous contributions to the literature. Second, our three main findings remain robust to

[44] The new rating agency *ARC Ratings*, an alliance of agencies from Brazil, India, Malaysia, Portugal, and South Africa, wants to challenge the established agencies, in particular by emphasizing their multinational background (see http://www.arcratings.com, accessed 12 May 2014). Similarly, UCRG, headquartered in Hong Kong, is a new joint venture of agencies based in China, Russia and the United States (http://en.dagongcredit.com/content/details20_7016.html; accessed 12 May 2014). Annette Heuser, executive director of the Germany-based Bertelsmann Foundation, speaks of "a boom in new rating agencies" and promotes the establishment of an international non-profit credit rating agency called Incra (see *Financial Times* article at http://www.ft.com/intl/cms/s/0/4140e388-cfc1-11e3-9b2b-00144feabdc0.html?siteedition=intl#axzz30r3qAT6O, accessed 30 April 2014).

augmenting the specification with additional fixed effects. This shows that these results are not driven by factors that are time-invariant in our sample such as the advantage that the United States enjoys with the US dollar being the world's reserve currency. Third, we run agency-specific regressions that allow for differential weights and assessments across agencies, the results of which confirm our main conclusions. Fourth, where applicable we separately assess the role that the nationality of an agency's main owner might play compared to headquarter location. Fifth, we show that our findings are not caused by selection into the sample of rated countries.

Finally, we explore in greater detail the channels through which cultural distance has an effect on rating outcomes. Employing data on the existence of foreign offices, it seems that the bias is not driven by superior information. Instead, we provide evidence for a limited sample of European countries, for which data on bilateral trust are available, that in the case of ratings cultural bias is associated with differences in relative risk perceptions related to trust. Using quantile regressions, we also demonstrate that the effect of cultural distance is significantly negative across all quantiles of the rating distribution and is more pronounced at lower quantiles.

The article proceeds as follows. In Section 2, we introduce the credit rating agencies and compare their key characteristics. Section 3 summarizes the existing literature on sovereign ratings and highlights the gaps in the literature that this article addresses. In Section 4, we discuss the mechanisms that could lead to a home bias in sovereign ratings and present our hypotheses. Section 5 introduces the data and econometric methods. In Section 6, we put our hypotheses to an empirical test and present our results. Finally, Section 7 summarizes, concludes, and outlines policy implications.

1. The agencies

Credit rating agencies are private companies that assess the default risk of all types of bonds. There are about 150 agencies operating in the rating business worldwide (White 2010; De Haan and Amtenbrink 2011). Of these, most agencies are active in a narrow national or regional market and focus solely on corporate ratings. Only a small number of agencies issue sovereign ratings, eight of which provide sovereign ratings on Bloomberg: *Capital Intelligence* (CI), *Dagong Global*, *Dominion Bond Rating Services* (DBRS), *Fitch Ratings*, *Japan Credit Rating Agency* (JCR), *Moody's Investors Service*, *Rating and Investment Information* (R&I), and *Standard & Poor's* (S&P). *Feri EuroRating Services* assigns ratings to 56 countries (and kindly provided us their data). Our study covers these nine agencies based in six countries.[45]

Sovereign ratings, often used synonymously with the terms "sovereign credit ratings," "sovereign debt ratings," or "sovereign risk ratings," are assessments of a country's creditworthiness. CI, for example, defines sovereign credit ratings as an indicator of "the ability and willingness of sovereign governments to repay existing and future commercial debt obligations on time and in full."[46] The rating scales follow letter designations and differ only marginally in terms of notation across agencies (see Appendix II: A0 for details). The best rating issued by most agencies is the grade "AAA," which is assigned to sovereigns with the lowest probability of default. As of June 2013, Switzerland, Finland, Liechtenstein, Luxembourg and Norway are the only sovereigns that receive the top rating from all of the agencies that assign a rating to them. Ratings of "BB" or lower are usually considered to be of "junk status," i.e., to have a high expectation of default.

Table II.1 provides an overview of the nine agencies covered in our study. While the corporate history of S&P dates back to the 19th century, the smaller agencies are relatively new actors in the rating business. DBRS is the oldest of the small agencies, having been active since 1976. Most of the

[45] We thus do not cover very small agencies. The U.S.-based agency *Egan-Jones Ratings Company*, for example, provides ratings for 19 sovereigns only (see http://www.egan-jones.com/; accessed 24 September 2013). Another example is the Ukrainian rating agency *Credit-Rating* that only issues sovereign ratings for member countries of the Commonwealth of Independent States (see http://www.credit-rating.ua; accessed 17 September 2013).

[46] See CI's company website available at http://www.ciratings.com/page/rating-methodologies/sovereign-ratings (accessed 18 October 2013). Sovereign ratings are not to be confounded with "country risk ratings," which take a broader stance. Euromoney Country Risk, for example, evaluates the general risk of an investment in a country (details available at http://www.euromoneycountryrisk.com/; accessed 12 May 2014).

smaller agencies started to rate sovereigns in the late 1990s when the demand for these ratings increased strongly as a larger number of countries started to issue sovereign bonds. The nine agencies under analysis vary greatly in size. The three large U.S.-based agencies, Moody's, S&P and – to a lesser extent – Fitch, are the dominant players in the global rating market. They have several thousand employees each, of which more than a thousand are involved with ratings (White 2010). All three are not only physically present in their home country, but possess between 24 and 34 offices in other countries. The other six agencies employ a much smaller number of people, and only possess between one (JCR) and five (Feri) foreign offices. Nevertheless, the interest in these smaller agencies is increasing – not only in the financial sector. Downgrades by Dagong in particular receive significant media attention.[47] Moreover, all of the agencies are officially recognized in the European Union, Japan and/or the United States.

The agencies also differ considerably with regard to their ownership structure. Banks and financial institutions are among the most important shareholders of most credit rating agencies. Feri, Moody's, S&P's mother company McGraw Hill, JCR and R&I all have one or more financial institutions among their major shareholders. For example, the Vanguard Group, the Bank of New York Mellon Corp. and BlackRock Inc. are shareholders of both Moody's and S&P. The vertical and horizontal cross holdings have led to concerns about the influence of banks as shareholders if they are also invested in rated securities.[48] Dagong and DBRS, in turn, are owned by private individuals, the former Chinese government official Guan Jianzhong and Canadian businessman Walter Schroeder, respectively.

In two cases, the home country as determined by the agency's headquarters is not identical to the home country as defined by the citizenship of the agency's major shareholders. While the U.S.-

---

[47] See, for example, articles on the websites of *The Economist* (http://www.economist.com/blogs/buttonwood/2011/08/debt-ceiling-crisis-1; accessed 13 November 2013), the *Wall Street Journal* (http://blogs.wsj.com/moneybeat/2013/10/17/chinas-dagong-takes-aim-at-u-s/?KEYWORDS=dagong; accessed 13 November 2013) and *CNN* (http://edition.cnn.com/2011/BUSINESS/08/02/china.us.rating/; accessed 13 November 2013)

[48] The European Commission speaks of "shareholders that sometimes overlap" and "risk of conflicts of interest that could affect the quality of rating." Its new rules specifically "require CRAs to disclose publicly if a shareholder with 5% or more of the capital or voting rights holds 5% or more of a rated entity" and "prohibit ownership of 5% or more of the capital or the voting rights in more than one CRA" (see European Commission MEMO/13/13 available at http://europa.eu/rapid/press-release_MEMO-13-13_en.htm, accessed 11 December 2013).

based agency Fitch belonged to 100 percent to the French holding company Fimalac until 2006 (which still holds 50 percent of the shares), Cyprus-based CI is owned by a privately-owned Kuwait-based company and three private individuals.

Finally, there are huge discrepancies with respect to the country coverage of the rating agencies. While S&P has the widest coverage with 125 countries (and territories), DBRS covers the least (25). There are not only substantial differences with respect to the set of countries covered, but also striking differences with respect to the ratings assigned. Figure 1 shows world maps graphically representing the sovereign ratings assigned by Moody's and Dagong. They show, for example, that Moody's rates the United States with the top-notch "AAA," while Chinese Dagong assigns an "A" only, i.e., five points lower (see Appendix II: A1 for maps for the remaining agencies). Table II.2 compares the (average) rating assigned to each home country by its respective home agencies to the average rating received from all other agencies. As of June 2013, six out of eight home countries receive a better rating from their home agencies. Does the literature provide an explanation as to why these differences in rating assignments exist across rating agencies? This is what we turn to next.

2. What the previous literature examines

In their seminal paper, Cantor and Packer (1996) investigate the determinants of rating assignments by the US agencies Moody's and S&P. They identify a set of key variables, confirmed by Afonso (2003), which contains GDP per capita, GDP growth, past default experience, the inflation rate, external debt and an industrialized country dummy. However, the two agencies are found to put different weights on the individual factors. Similarly, Hill et al. (2010) find significant differences in the weights that Fitch, Moody's and S&P each assign to the various factors that determine sovereign ratings. By comparing the relevant factors, they identify a similar set of variables that are used as common determinants of sovereign ratings: GDP per capita, GDP growth and its square, past default experience, country risk rating, and risk premiums.

Other recent studies have focused on political factors as determinants of sovereign ratings. Starting with Archer et al. (2007), one strand of the literature examines a potential "democratic advantage" in sovereign ratings. While Archer et al. (2007) do not find empirical support for the suggested

positive link between ratings and democratic institutions, Beaulieu et al. (2012) find evidence for such a relationship when they account for the fact that democratic countries are more likely to have access to the international capital markets. Disentangling electoral democracy and political constraints, Cordes (2012) shows that the existence of contested elections does not matter for rating outcomes, but countries that impose more political constraints on the executive do receive better ratings. Along similar lines, Biglaiser and Staats (2012) find that countries' rule of law, judicial independence and protection of property rights all improve a country's rating outcome. Overall, political factors have been shown to play an important, though smaller, role compared to economic fundamentals (Haque et al. 1998; Archer et al. 2007). Among the political factors that seem to be considered in agencies' rating decisions are political business cycles (Block and Vaaler 2004; Vaaler et al. 2006; Biglaiser and Staats 2012) and executive party tenure (Archer et al. 2007).

The findings of the previous literature have two important caveats. First, most of the literature analyzes only sovereign ratings made by the big three U.S.-based agencies. Among the few exceptions, Alsakka and ap Gwilym (2010) compare rating decisions by Japanese agencies to those of the US agencies and Zheng (2012) analyzes differences between China's Dagong and S&P. Bartels and Weder di Mauro (2013) come to the surprising conclusion that the ratings assigned by Germany-based Feri to European countries are "tougher" than those issued by its US competitors. To the best of our knowledge, no empirical study so far has looked at a broad set of rating agencies to analyze the determinants of sovereign ratings. Second, although some studies find differences in rating assignments between agencies, the previous literature does not offer an explanation for these marked differences. The literature so far explained sovereign ratings r issued by a rating agency a based in home country j only with characteristics of the rated country i at time t:

$$r_{a,j,i,t} = f(e_{i,t}, p_{i,t}) \qquad (1)$$

where e stands for the economic fundamentals and p for the political fundamentals of the rated country. This approach ignores the potential role played by the location of the agency's headquarters and the nationality of its main shareholders. We aim to fill this gap.

3. Why the home country could matter

We argue that there are two potential sources of a home bias. First, political economy influences on the rating process could bias rating decisions in a way that favors the home country's economic and geopolitical interests. Governments and lobby groups might put pressure on rating agencies in order to advance the economic and geopolitical interests of the home country, leading to relatively better ratings for certain countries. Second, rated countries that are culturally more similar to the home country could obtain a higher rating. Extending equation (1) from above, we hypothesize that home-sovereign pair-specific variables $x_{j,i,t}$ explain sovereign ratings in addition to the economic and political fundamentals specific to country i:

$$r_{a,j,i,t} = f(x_{j,i,t}, e_{i,t}, p_{i,t}) \qquad (2)$$

A thorough understanding of the rating process is crucial to identify potential sources of home biases. For the most part, all agencies follow similar procedures to determine their ratings. The usual process that leads to a rating assignment can be grouped into four phases (see also figure in Appendix II: A2). In the first phase, *Rating Initiation*, the agency enters into an official agreement with the sovereign (in case of a solicited rating), assigns analysts to that particular sovereign and collects data. In the second phase, which we refer to as *Due Diligence*, the agency gathers further information by collaborating with the sovereign, which are subsequently analyzed. In the third phase, *Rating Assignment*, the analysts draft a preliminary report for the rating committee, which has the final say on the decision. The committee consists of several analysts, augmented with at least one senior director with managerial authority, who serves as chairperson. After the committee's decision, the sovereign gets notified and has the opportunity to appeal. Most agencies approve such a request if the sovereign provides new or additional information that the agencies consider relevant. In the final phase, *Rating Publication*, the agency publishes the final report and the corresponding rating and continuously monitors the sovereign's economic and political situation.[49] The knowledge about the rating process allows us to derive how economic interests, geopolitical alignment and cultural proximity could influence rating decisions.

[49] Still, there are several important differences in the rating process between the nine agencies under analysis as we summarize in Appendix II: A3. For example, while most agencies (e.g., Fitch, S&P) conduct several interviews with

3.1 Economic and Geopolitical Interests of the Home Country

Political economy influences to advance the home country's economic and geopolitical interests are more likely to occur during the later stages of the rating process. While analysts use (more or less) clearly defined quantitative and qualitative indicators for the initial rating proposal ("sovereign indicative rating level"), the composition of the rating committee leaves room for the members of the rating committee to deviate from an "objective" rating proposal. S&P, for example, speaks of "exceptional adjustment factors" (S&P 2012). The possibility of sovereigns to appeal after the initial rating decision provides further potential for interested parties to influence rating outcomes.

Governments have incentives to sway rating decisions. First, sovereign ratings determine the extent to which countries can access international capital markets, particularly in the case of emerging and developing economies (e.g., Cantor and Packer 1996, Reinhart 2002). Second, even for industrialized economies with well-established market access, rating downgrades can influence a sovereign's borrowing costs (Afonso et al. 2012). In particular, certain institutional investors are bound by their own charter or law to choose only assets above a certain critical rating threshold ("hardwiring of ratings").[50] For a sovereign, falling below this threshold would lead to a sudden drop in bond demand and consequently an increase in bond yields. Third, rating decisions can also be interpreted as a signal of leader performance. Despite the widespread public skepticism towards the agencies, downgrades can severely damage the reputation of a government. For example, the downgrades of France in 2012 and 2013 cast doubts over the optimistic predictions of President François Hollande's government concerning the future development of the French economy. According to the media, the downgrade by S&P constitutes "politically a heavy blow for

---

representatives of the sovereign, which usually involve personal meetings at relevant ministries and central banks, others do not (CI, Dagong, and Feri). Also, R&I is the only agency that does not provide the sovereign with the opportunity to pursue a rating appeal after being notified about the final rating decision. Concerning the surveillance of the credit rating, some agencies follow a fixed update interval, while others provide ongoing surveillance (e.g., S&P). See also Gaillard (2012: ch. 4) for a detailed description of the rating process of the three U.S.-based agencies.

[50] The US Securities and Exchange Commission (SEC), for example, restricts money market funds to purchase only securities rated by any two Nationally Recognized Statistical Rating Organizations (NRSRO) in one of the two highest rating categories. The US Department of Labor restricts pension fund investments to securities rated "A" or higher (Rule 2a-7 [10] of the Investment Company Act). The Dodd-Frank Act (S. 939A) requires federal agencies to review such problematic regulations.

the head of state."[51] Fourth, governments might also be interested in ratings of countries to which they have strong economic ties. Downgrades and potentially resulting instabilities may harm the home country's export interests and endanger the investments of domestic agents. Home-country governments may also have an interest in avoiding a destabilizing downgrade of geopolitically aligned countries, i.e., countries with which they collaborate in international fora or that are of military importance.[52]

Although governments have these motives to influence rating decisions, it is less obvious that they also have the political power to influence assessments provided by privately-owned credit rating agencies. With respect to international financial organizations, it is much more straightforward that political economy factors can affect economic assessments since governments are direct shareholders of these institutions (e.g., Dreher et al. 2008; Fratzscher and Reynaud 2011). However, there is a questionable dependence of agencies on governments, in particular their home government. In the European Union and the United States, only the ratings of officially recognized agencies can be used by companies to determine their capital requirements (White 2010; De Haan and Amtenbrink 2011). Dagong, for example, was denied recognition as a Nationally Recognized Statistical Rating Organization (NRSRO) in the United States in September 2010. The prospect of losing their recognition is an imminent threat to agencies. This is particularly the case for home-country agencies since corporate ratings in their respective home countries are usually a major source of their respective income.

There is anecdotal evidence that governments try to use their influence to impact rating decisions. Two weeks after the U.S.-based rating agency Egan-Jones downgraded the United States to "AA" in 2012, the US Securities and Exchange Commission (SEC) brought administrative action against the firm for alleged "material misstatements" during its application for regulatory approval in 2008. In this context, the owner of Egan-Jones, Sean Egan, stated that "[w]e are not going to be

[51] See an article by France's leading financial newspaper, *Les Échos*, available at http://www.lesechos.fr/economie-politique/politique/actu/0203115388893-degradation-de-la-note-de-la-france-un-nouveau-coup-dur-pour-l-executif-627380.php (accessed 30 November 2011, own translation of the quote).

[52] In this regard, DiGiuseppe et al. (2012) show that countries with affordable credit access have a lower probability to experience civil conflict.

intimidated by anybody from issuing timely, accurate ratings."[53] In a similar case, S&P called a $5 billion lawsuit against itself "a retaliation for its 2011 decision to strip the country of its AAA credit rating."[54] Government ties are more directly visible for Chinese Dagong, which entertains strong relations with several Chinese government institutions (see Ling 2012 for a discussion). With regard to corporate ratings, Ferri et al. (2013: 774) note that, in comparison to the large US agencies, the smaller Asia-based agencies are "perceived to be generally less independent because they are often subject to a possible conflict of interest stemming from the ownership structure, and are prone to a home-country bias."

Beyond direct pressure from governments, other interested parties within an agency's home country could potentially influence rating outcomes. First, since many banks and financial institutions are among the major shareholders of rating agencies, they could have the leverage to directly or indirectly exert an influence on the agencies' decision-making to protect the value of their bond holdings. According to Shin and Moore (2003), there are indications that the Japanese rating agencies are more vulnerable to influence from their shareholders than their US counterparts. They cite a report by the Japan Center for International Finance, according to which "[t]he composition of the shareholders of rating agencies may impair the impartiality of ratings" (Shin and Moore 2003: 331). Shareholders might try to obtain a preferential treatment of countries where they are exposed to large risks, e.g., where they have a large amount of outstanding loans. Second, agency staff or persons close to them could potentially have personal monetary investments in the respective country under assessment. A home bias might arise because these actors, like other investors, are more likely to hold larger stakes in the domestic economy or economies with close ties than somewhere else (e.g., French and Poterba 1991; Tesar and Werner 1995). However, corporate governance rules forbid analysts themselves to hold stakes in investments in an asset class under evaluation. Third, analysts at rating agencies might be influenced by future career concerns; the prospect of a new job in the banking sector, easily possible because of similar job requirements, might seem attractive. In this context, Bar-Isaac and

[53] See, for example, an article on the website of *The Wall Street Journal* available at http://online.wsj.com/news/articles/SB10001424052702303513404577354023825841812 (accessed 18 November 2013).

[54] See, for example, an article by *Reuters* available at http://www.reuters.com/article/2013/09/03/us-mcgrawhill-sandp-lawsuit-idUSBRE98210L20130903 (accessed 15 December 2013).

Shapiro (2011: 120) speak of a "revolving door" that connects rating agencies and investment banks. One might argue that analysts could intendedly or unintendedly take the concerns of their potential future employers into account and rate countries higher in which home-country banks are strongly invested.

Finally, beyond political economy influences, national sentiments of agency staff could potentially cause home biases. Morse and Shive (2011) provide evidence that patriotism explains (parts of) the home bias in equity. Similar to "patriotic investors," employees of rating agencies might be reluctant to downgrade the home country or another sovereign either because they believe that this decision could have a detrimental impact on their home country or if they are – in the words of Morse and Shive (2011: 411) – "blinded by patriotic loyalty." If such an effect exists, it should be more pronounced in agencies where national sentiments are anchored in the corporate culture. Ling (2012), for example, describes the Chinese agency Dagong as a "patriotic rating agency." Ling refers to the company's website which states that the agency aims to promote the patriotism of its employees. Taken together, there are theoretical arguments that economic ties and geopolitical interests of the agency's home country could be related to rating outcomes.

3.2 The Role of Cultural Distance

Various studies discuss the role of cultural distance in financial decision-making. Guiso et al. (2009: 1095) identify "perceptions rooted in culture" as important determinants of bilateral investments. Grinblatt and Keloharju (2001: 1072) hypothesize that "familiarity-related effects could be the major contributor to home bias." Indeed, their empirical analysis shows that culture influences stockholdings of both private and corporate investors. Empirical evidence also suggests that firms favor culturally closer overseas listing venues (Sarkissian and Schill 2004). With respect to syndicated bank loans, Giannetti and Yafeh (2012) show that lenders treat borrowers from countries that are culturally more distant as less reliable. They receive smaller loans, pay higher interest rates and are more often required to provide a third-party guarantee. Given this evidence, it would not be surprising if cultural distance also affects decision-making at rating agencies.

Why would agencies assign better ratings to culturally closer and thus more familiar countries? To answer this question, it is important to remember that rating agencies have to base their

assessments on limited and incomplete information. They acquire this information either from publicly available sources or through communications with the sovereign directly ("Due Diligence"), for example, at meetings with officials of the country's ministries or the central bank. The agencies have to cope with concerns regarding the reliability and accuracy of the acquired information. We discuss three lines of reasoning why cultural proximity (familiarity) might affect rating decisions. These are based on information, differences in risk perceptions, and taste-based discrimination.

First, an information-based theory of cultural distance would assume that a home bias can be the result of completely rational behavior.[55] Giannetti and Yafeh (2012) argue that banks collect less information about culturally more distant borrowers due to higher costs of information gathering and thus consider them as riskier. Similarly, rating agencies face a trade-off between the benefit of acquiring additional information and the transaction costs this would incur. Linguistic differences raise transaction costs by increasing the difficulties of direct communication and the ease of translation (see also Melitz 2008). This can cause agencies to collect less information overall. Less information can then translate into lower ratings since predictions of the sovereign's liquidity to serve its debts are less precise and thus imply a higher probability of defaulting.[56] Due to this information-cost trade-off, it can be rational for agencies to assign lower ratings to less familiar countries (for a similar argument see Chan et al. 2005). Van Nieuwerburgh and Veldkamp (2009) demonstrate in a more general setting that a tiny information advantage is enough to generate a significant home bias.

[55] See Grinblatt and Keloharju (2001) for a similar argument.

[56] Assume that a rating agency estimates the liquidity L of two sovereigns i, with $E[L_i] \sim N(\mu, \sigma_i^2)$. A sovereign enters a state of default if $L_i < z$. Thus, the probability of default is $P(L_i < z) = F\left(\frac{|z-\mu|}{\sigma_i}\right)$. Assume that two sovereigns A and B have the same expected value μ, but sovereign B is culturally more distant to the home country of the rating agency. As a result of higher transaction costs, the agency collects less information about sovereign B, which implies that its prediction of L_B is less precise compared to that of L_A, i.e., $\sigma_B^2 > \sigma_A^2$. Thus, $F_B\left(\frac{|z-\mu|}{\sigma_B}\right) > F_A\left(\frac{|z-\mu|}{\sigma_A}\right)$ for all $z < \mu$, i.e., the predicted default probability is higher for the culturally more distant sovereign. A similar argument is made by Gehrig (1993) and Brennan and Cao (1997), whose noisy rational expectations model shows that a home bias in international equity investments exists under the assumption that domestic investors have more precise information than foreign investors.

The second explanation for the role of cultural distance focuses on differences in risk perceptions, in particular with regard to relative optimism and trust. Following the literature on an "optimism bias," a home bias does not require that the actors actually possess more information; it suffices that the actors *perceive* the information differently. Kilka and Weber (2000) find that that people hold more "optimistic" expectations of domestic investments as they feel more "competent" about investment possibilities at home.[57] Similarly, French and Poterba (1991) explain the home bias in portfolio holdings with domestic investors' more optimistic expectations about domestic stock returns compared to foreign stocks. The existing literature suggests that trust rooted in culture is a potential cause of these differences in perceptions. Guiso et al. (2009) find that cultural distance translates into lower levels of bilateral trust, which then translates into less economic exchange between countries. Their analysis of bilateral investments concludes that "cultural effects are not limited to unsophisticated consumers, but are also present among sophisticated professionals" (p. 1098).

In the rating process, trust between the agency's home country and the rated country could influence risk perceptions of the agency staff and thus lead to an "optimism bias." For example, analysts could perceive the same economic information from a more familiar source as more reliable and its bonds as less risky. Beyond that, bilateral trust might not only matter for how analysts perceive the available information about the sovereign's *ability* to pay, it could also affect beliefs about a sovereign's *willingness* to pay its debt. This is important since countries commonly default on their debt for reasons other than insufficient liquidity (see historic evidence in Tomz and Wright 2007; Reinhart and Rogoff 2009). A government may decide to default for opportunistic reasons if the domestic political costs to the government of raising funds weigh higher than those caused by a default. With this in mind, it seems possible that rating analysts evaluate a government's willingness to pay more optimistically if the level of bilateral trust is

[57] For related empirical evidence of an "optimism bias," see Shiller et al. (1996) for a comparison between the stock market expectations of Japanese and American investors, and Strong and Xu (2003) for fund managers' views on the prospects of international equity markets.

higher.[58] Taken together, as cultural proximity relates to relative optimism and trust, it could also lead to better ratings of culturally closer countries.

A third potential reason for the role of cultural distance is direct discrimination of certain ethnicities or races. Building on Becker (1961) and Stiglitz (1973), we define discrimination in our context as behavior that treats two sovereigns with the same economic characteristics differently based on ethnic or racial differences. Taste-based discrimination may negatively influence how creditworthy borrowers are perceived to be (Giannetti and Yafeh 2012). Ravina (2008) provides empirical evidence that race affects credit conditions. Rating agency staff may exhibit similar behavior.

3.3 Hypotheses

Ideally, competition and concerns over reputation should incentivize agencies to publish accurate and unbiased ratings. The detailed arguments above, however, provide theoretical explanations why the home country of rating agencies might lead to biases. The extensive discussion enables us to formulate the following testable hypotheses about potential political economy influences and the role of culture in rating decisions:

Hypotheses: *Controlling for economic and political fundamentals of rated countries, a rating agency assigns a better rating to*

(1) its home country.

(2) countries in which the home country has larger economic interests.

(3) countries geopolitically aligned with the home country.

(4) countries that are culturally closer to the home country.

[58] See also Bergh and Bjørnskov (2014) for a similar argument based on country-level rather than bilateral trust.

Each hypothesis is tested against the null hypothesis that ratings are not affected by these home-country specific factors.

4. Data

4.1 Dependent Variable: Sovereign Ratings

Our dependent variable is a country's sovereign rating provided by one of nine rating agencies: CI, Dagong, DBRS, Feri, Fitch, JCR, Moody's, R&I, and S&P (see again Table II.1 for an overview). We follow the literature and examine the determinants of a sovereign's long-term foreign-currency rating, i.e., ratings for government bonds that are issued in a foreign currency and have a maturity of more than one year. We retrieve daily information on sovereign ratings by most agencies via Bloomberg (see Appendix II: A4 for details). The information on ratings published by Feri and Fitch is obtained directly from the agencies. We take the monthly average of the assigned ratings since our background research reveals that some agencies only review their ratings on a monthly basis (see again Appendix II: A3). Moreover, the highest frequency for which the explanatory variables are available is also monthly.[59] We obtain an unbalanced panel as each agency assigns ratings to different sets of countries over varying periods of time (see Appendix II: A5 for details).

For our empirical analysis, all ratings have been translated to a 21-point scale in accordance with the literature (see Hill et al. 2010 for a similar approach). This means that we assign the highest value of 21 for an "AAA" rating. "C" and "D" in turn are translated into a value of one.[60] The pair-wise correlation between sovereign ratings from the nine credit rating agencies under analysis ranges between 0.784 (CI-Feri) and 0.987 (Fitch-S&P).[61]

[59] Most of the literature uses yearly averages or year-end ratings. However, this results in a loss of information, most importantly since rating changes within a year are ignored. Moreover, sovereigns often receive their first rating in the course of a year and ratings can be withdrawn within a year. The usage of monthly data mitigates these problems compared to yearly averages or a simple extrapolation to the end of the year. See also footnote 33.

[60] As there are slight differences in the rating scale across rating agencies, please refer to Appendix II: A0 for a full translation table.

[61] See Appendix II: A6 for details. Gaillard (2012: ch. 6.3) provides a detailed discussion of the cases where even the ratings of the U.S.-based agencies show considerable differences.

4.2 Control Variables

We build on and combine the sets of variables employed in Archer et al. (2007) and Hill et al. (2010) to control for those country-specific economic and political factors that should "objectively" determine the ability of a country to repay its debt.[62] Comparing the factors that the nine agencies communicate in official documents (see Appendix II: A7) with those employed in the literature, there is a large overlap but also an apparent lack of control for conflict risks in the previous literature, which is why we add further variables in this category (see also Gaillard 2012, who refers to "event risk" as one factor applied by Moody's).

To capture the sovereign's **domestic economic performance**, we employ the country's logged *GDP per capita* (in constant 2000 US dollars), *GDP growth* rate (including a squared term) and *inflation* rate (based on consumer prices). Following Archer et al. (2007), we also control for the sovereign's *natural resources* measured as total natural resource rents as a percentage of GDP. Logged *population* size is added as a control variable since larger countries possess on average a more diversified economy and are thus less affected by external shocks. All data are obtained from the World Bank's World Development Indicators (WDI).[63] To account for a sovereign's **financial stability and fiscal performance**, we control for a country's gross government debt-to-GDP ratio (*government debt*) and its change over time (*change in government debt*). Two dummy variables account for past experiences with defaults (*default*). The first variable takes a value of one if the sovereign has experienced a sovereign debt crisis or restructuring since 1970. The second variable takes a value of one if the sovereign has defaulted or restructured its debt in the last five years. Data come from the IMF (Abbas et al. 2010; Laeven and Valencia 2012). We add three variables to account for a sovereign's **external performance**. *Trade openness*, i.e., the sum of the rated country's

[62] In contrast to Archer et al. (2007), we do not include variables that measure executive party tenure and undivided government since these variables contain too many missing values and would thus substantially reduce our sample size (data from Beck et al. 2001). We explicitly acknowledge that the agencies do in practice augment these third-party variables with their own forward-looking forecasts, which cannot be controlled for. However, we argue that these constitute outcome variables rather than controls and should thus be considered as "bad controls" in the terminology of Angrist and Pischke (2008). In fact, the forward-looking assessments should mostly be affected by potential cognitive biases, as they require and leave more room for subjectivity.

[63] Data are available at http://data.worldbank.org/ (accessed 29 April 2014).

exports and imports, *current account balance*, and debt owed to nonresidents (*external debt*), where all three variables enter as a share of GDP (data from WDI).

We control for eight measures of a sovereign's **political and institutional performance**. First, a sovereign's regime type is captured using the *polity* 2 variable from the Polity IV dataset (Marshall et al. 2013). Second, we include an *election* dummy that is coded as one if elections were held during the last 12 months. Third, *years in office* captures the number of years the chief executive has been in office as of January 1st. Fourth, a dummy for executive ideology (*left government*) is coded as one if the leader's party is considered as communist, socialist, social democratic or other left-wing (all). Fifth to eighth, we control for a country's *rule of law, absence of internal conflict*, *absence of external conflict* and *absence of military in politics*. These variables are drawn from the Database of Political Institutions (Beck et al. 2001) and the International Country Risk Guide (ICRG).[64] Finally, we also include a dummy variable whether a country is a member of the Eurozone.

We lag all time-varying variables for two reasons. First, the process of incorporating new data and political developments takes some time. Second, agencies have an incentive not to change their ratings too often, as very frequent changes would cast a bad picture on their long-term analytical skills. On average, agencies would rather wait for bad (or good) news to be confirmed by other sources than to react immediately to changes in their indicators. In most cases, we compute the moving average of each variable over the previous 12 months. In other cases, in particular for more volatile variables capturing the current economic situation, we calculate the average over the last 36 months to cancel out pure business-cycle effects and random short-term fluctuations that should not influence long-term debt repayment abilities (see Block and Vaaler 2004 for a similar approach). Appendix II: A8 provides precise definitions of all variables employed, their sources and details on the computation of moving averages. Appendix II: A9 shows the corresponding descriptive statistics.

[64] Data are available at http://www.prsgroup.com/icrg.aspx (last accessed 14 August 2013).

4.3 Variables of Interest

We use eight variables to test the presence of a home bias. As we will argue below, none of these variables should influence sovereign ratings, controlled for the variables introduced in the last subsection. Starting with the most obvious test for a home bias, the *same country* dummy takes a value of one if the rated country is the home country of the rating agency. The home country is defined as the country in which the headquarters of the agency is physically located.[65] We also refer to home bias if rating agencies assign relatively better ratings to countries with close economic ties, geopolitically-aligned countries and countries that are culturally closer. In the following, we introduce the variables employed in these three categories.

Economic interests. We account for two variables that capture economic links between the rated countries and the home country of the rating agency. First, *export interests* are measured by the rated country's share of the home country's total exports. Export data are obtained from UN Comtrade and have been accessed via the World Bank's World Integrated Trade Solution (WITS) software.[66] While a sovereign's access to foreign currency should matter for its ability to pay back its debt and is covered by our control variable *current account balance*, the sovereign's relative importance as an export market for the home country of the rating agency should, all else being equal, be unrelated to rating outcomes.

Second, we test for the role of *bank exposure*. The Bank for International Settlements provides data on bank exposure of all banks headquartered in most of our home countries.[67] It contains detailed information on all foreign claims to the public sector, banks, the non-bank private sector, and miscellaneous other claims. In addition, it covers potential exposures from derivatives contracts, guarantees and credit commitments. As our measure of *bank exposure*, we compute the rated country's share of the total risk exposure of all home-country banks.

---

[65] Using an ownership-based definition, we will also present results where we code home countries according to the nationality of the major shareholder(s). Both definitions coincide in most cases; CI and Fitch are the only exceptions (see again Table 1 for details).

[66] The online tool is available at http://wits.worldbank.org (accessed 2 May 2014).

[67] Unfortunately, no data are reported from Cyprus (CI) and China (Dagong). Data are available at http://www.bis.org/statistics/consstats.htm (accessed 2 May 2014).

Geopolitical alignment. We use two measures to test whether countries that are geopolitically aligned with the home country of the rating agency receive relatively better ratings. First, we use bilateral voting alignment in the UN General Assembly as a proxy for *geopolitical alignment* between the rated country and the home country of the rating agency. This is defined as the share of votes in which the rated country and the home country exhibit the same voting behavior, i.e., both voting yes, both voting no, or both abstaining (data from Strezhnev and Voeten 2012). Two voting blocs can be identified in the post-Cold War period: a Western bloc around the United States on the one hand and an "counterhegemonic bloc," which includes China, on the other (Voeten 2000). As an illustrative example, consider the case of Malaysia, which is strongly aligned with China and receives on average a two-point better rating from Dagong compared to the three big US agencies. Measures of UN voting alignment are widely used in the literature to measure bilateral affinity (e.g., Barro and Lee 2005; Dreher and Gassebner 2008; Qian and Yanagizawa 2009). For the US agencies in our sample, we employ a country's share of total US military aid as a second measure of geopolitical alignment. This variable serves as a proxy for the strategic importance that the United States assigns to these countries.[68]

Cultural distance. We use three measures of cultural distance. The first variable is a simple dummy that takes a value of one if home and sovereign share the same official language (see also Guiso et al. 2009; data from Mayer and Zignago 2011). Moreover, we follow several studies that use more comprehensive measures of cultural distance (e.g., Guiso et al. 2009; Giannetti and Yafeh 2012). Specifically, we use two dimensions from Kolo's (2012) distance-adjusted ethno-linguistic fractionalization index (DELF).[69] The second variable *Cultural distance (language)* measures linguistic differences based on language trees from the Ethnologue project, which classifies 6,656 distinct languages into families and branches due to their linguistic origin (Lewis 2009). As

[68] Data are available at the USAID website at http://gbk.eads.usaidallnet.gov/data/detailed.html (accessed 2 May 2014).

[69] The approach in Kolo (2012) builds on the ethno-linguistic fractionalization index (ELF, Alesina et al. 2003). The cultural distance between two countries A and B is calculated as $DELF = \left(1 - \sum_{k_A=1}^{K} \sum_{k_B=1}^{K} p_{k_A} p_{k_B} \hat{s}_{k_A,k_B}\right) \cdot 100$. The calculation weighs each distinct group k within countries A and B by their relative group size p_k. The product of the weights is multiplied with a similarity parameter $\hat{s}_{k_A,k_B}$, which varies between zero and one and takes the value of one if individuals belong to the same cultural group. The resulting $DELF$ value gives the expected similarity between two randomly drawn individuals.

outlined before, larger cultural distance could be related to reduced information gathering and less optimistic risk perceptions.

The third measure, *cultural distance (ethno-racial)* is derived from a biological taxonomy of species, based on genealogical relatedness. Kolo (2012) assigns six-letter codes, which are based on race, skin pigmentation and ethnic origin and allow more differentiation between genetic groups than mere genetic distance (as in Spolaore and Wacziarg 2009, for example). Ethno-racial distance can be seen as a proxy for economic discrimination based on race or ethnicity; however, it might also be related to trust towards the rated country.

Appendices A8 and A9 provide detailed definitions, sources and descriptive statistics for all variables of interest. Appendix II: A10 shows simple correlations with the average sovereign rating, and partial correlations holding GDP or the debt ratio constant. We now turn to our econometric estimations.

5. Econometric analysis

To test for the existence of a home bias in sovereign ratings, we estimate the determinants of sovereign ratings $r_{a,j,i,t}$. Specifically, we estimate the following regression equation using ordinary least squares (OLS):

$$r_{a,j,i,t} = \beta x_{j,i,t} + e'_{i,t}\gamma + p'_{i,t}\delta + \alpha_{a,j} + \tau_t + \varepsilon_{a,j,i,t} \qquad (3)$$

where $x_{j,i,t}$ is a country-pair-specific variable of interest, $e_{i,t}$ and $p_{i,t}$ are vectors that contain sovereign-specific economic and political control variables, $\alpha_{a,j}$ and τ_t are agency- and period-fixed effects, respectively, and $\varepsilon_{a,j,i,t}$ is the error term.[70] Error terms may be correlated at both the

[70] We use agency-fixed effects rather than home-country-fixed effects to account for differences in the average rating level that can exist between the agencies from one country. For example, in the United States, Moody's could be systematically more pessimistic than S&P (or vice versa).

agency-time and sovereign level. Thus, it is advisable to use two-way clustering on both dimensions.[71] The time period we analyze runs from January 1990 to June 2013.[72]

Our identification strategy exploits three sources of variation in the dependent variable: (i) differences in the rating levels between rated countries, (ii) changes in ratings within rated countries, and (iii) differences in ratings between agencies for the same rated country at a specific point in time. Since ratings are rather sticky and exhibit little variation for some countries, we choose equation (3) as our baseline to be able to identify our coefficients based on all three sources of variation. To alleviate concerns about unobserved (time-invariant) characteristics of rated countries, we also show results for a specification that adds sovereign-fixed effects to equation (3):

$$r_{a,j,i,t} = \beta x_{j,i} + e'_{i,t}\gamma + p'_{i,t}\delta + \alpha_{a,j} + \eta_i + \tau_t + \varepsilon_{a,j,i,t} \qquad (4)$$

For the time-varying variables of interest $x_{j,i,t}$, we replace η_i with stricter agency-sovereign-pair-fixed effects $\eta_{a,j,i}$. Identification of β in equation (4) rests on the variation caused by (ii) and (iii). The most restrictive specification is to use agency- and sovereign-time-fixed effects, which leads to identification of β being based solely on variation caused by (iii). Using the previous notation, we obtain

$$r_{a,j,i,t} = \beta x_{j,i} + e'_{i,t}\gamma + p'_{i,t}\delta + \alpha_{a,j} + \omega_{i,t} + \varepsilon_{a,j,i,t} \qquad (5)$$

For the time-varying variables of interest $x_{j,i,t}$, we replace $\alpha_{a,j}$ and $\omega_{i,t}$ by stricter agency-sovereign-pair-fixed effects $\omega_{a,j,i,t}$. Equation (5) minimizes the potential omitted variable bias, but identification is based only on variation across nine agencies. Thus, for any $x_{j,i}$, β is identified only by observations of countries rated by more than one agency at the same point in time where we observe a difference in ratings between these agencies. If our variables of interest pass this test, this provides strong evidence in favor of the respective hypothesis. The coefficients and standard

[71] We thank an anonymous referee for this suggestion. When we clustered at the most aggregate level of clustering in earlier versions of this paper, i.e., at the sovereign level, our results were very similar (see Cameron et al. 2011 on both approaches).

[72] Using monthly ratings is more efficient as it makes use of all available variation in the data. This does not deflate panel-robust standard errors compared to yearly data since they depend on the number of clusters $C \to \infty$ rather than the number of observations $N \to \infty$ (compare Cameron et al. 2011). All the same, our results are virtually unchanged when using yearly averages as can be seen in Appendix II: A11.

errors from equation (5) should be interpreted with caution, however, as this is can be an overly restrictive test. If equation (5) leads to insignificant results, this can be due to a lack of variation as outlined above. Also, the estimated β might not be representative for the entire sample in case of heterogeneous effects. For these reasons, we take equation (3) as a baseline specification and show the results based on equations (4) and (5) for comparison.

In addition to our regression results for the full sample, we also show results for a sample restricted to the time after the onset of the Global Financial Crisis (GFC). We take September 2008 as the starting point for this period of increased uncertainty. In this month the bankruptcy of Lehmann Brothers and American International Group (AIG) took place (for a detailed discussion of these events, see Mishkin 2011). Home-country influences on sovereign ratings could have become more pronounced following the onset of the crisis. Investors became more risk-averse during the crisis; specifically, doubts about the use of sovereign bonds as quasi risk-free assets in financial models have risen. Additionally, politicians, journalists and economists intensified their critique of the agencies' decision-making and increased the public pressure not to downgrade their countries.

Ordinary least squares treat the dependent variable as cardinal. This implies that the difference between an "AA" and an "AA+" rating, for example, is the same as between "BB" and "BB+." In most settings, this choice has little effect on the direction and significance of variables if the number of response categories is sufficiently large (see Ferrer-i-Carbonell and Frijters 2004, for example). Nonetheless, we will also show the results of an ordered probit model for the discrete 21-step rating at the end of a month. To keep the discussion concise for the reader, we focus on the direction of the effect and the significance levels as marginal effects are conditional on each respective rating level.

5.1 Main Results

To reduce clutter, we do not display the results of a baseline specification that excludes our variables of interest $x_{j,i,t}$. The interested reader can find them in Appendix II: A12. Its results are in line with former studies focusing on fewer agencies and shorter time periods. The objective country-specific controls alone explain 86 percent of the variation in sovereign ratings, as indicated by the adjusted R-squared value. It is reassuring that our baseline model thus explains a large share of the variation in sovereign ratings from this broad set of agencies.

To test whether sovereign ratings show evidence for home biases, we add one variable of interest at a time to the baseline specification introduced in the previous subsection. Each cell in Table II.3 refers to one independent regression and shows the coefficient of the respective variable of interest. We show results for the full sample (columns 1 and 3) and the GFC sample (columns 2 and 4) for both least squares and ordered probit.[73] As can be seen from the positive coefficient on *same country* in column 1, which is significant at the five-percent level, agencies assign a rating that is 0.95 points higher to their home country than to other countries with the same characteristics. In other words, the rating of the home country is on average almost one point higher than what would be justified by how the agencies assess and weigh the economic and political fundamentals of other rated countries. The favorable treatment of the home country becomes more pronounced during the GFC period; the coefficient rises to 1.7 and becomes significant at the one-percent level (column 2). The ordered probit regression results in columns 3 and 4 confirm the significant effect of *same country*, at the one-percent level. Taken together, this is strong evidence in favor of the first home-bias hypothesis.

Do rating agencies also exhibit a home bias by favoring countries that are economically, geopolitically or culturally aligned with their home country? Starting with the measures of economic ties, our results partly support the notion that rating agencies favor countries economically aligned with their home country. While *export interests* does not gain significance in

[73] For the interested reader, Appendix II: A13 shows the results for the control variables for each regression in column 1, Appendix II: A14 displays the corresponding adjusted R-squared values and number of observations, and Appendix II: A15 shows separate results for the pre- and post-GFC sample. Appendix II: A15 also tests whether the results are affected when we add the *same country* dummy in order to test whether the results for the remaining variables of interests are not just capturing the same-country effect. As can be seen, this is not the case.

any of the specifications in Table II.3, the second variable that proxies for economic ties, home-country *bank exposure*, is positive as expected and significant at conventional levels in all estimations. The size of the effect is again larger after the onset of the GFC. According to column 2, an increase of the sovereign's share in the home country's bank exposure by 22 percentage points is related to a rating improvement of one point on the rating scale.

While the coefficient on *geopolitical alignment (UN)* does not reach statistical significance at conventional levels in columns 1-3, the positive coefficient turns significant at the one-percent level in the GFC sample when using ordered probit (column 4). The effect of *US military interests (aid)* on the rating behavior of the US agencies is positive and statistically significant in all specifications. We refrain from overinterpreting this latter finding here as it is not supported in the agency-specific regressions below (see Section 6.2). Taken together, there is only weak evidence that geopolitical ties between home and sovereign decisively influence rating outcomes.

We now turn to the role of cultural ties between the sovereign and the home country of the rating agency. The coefficient on *common language* is positive as expected and statistically significant at least at the five-percent level in all specifications. Countries that share a common language with the home country on average receive a rating that is 0.8 points higher (column 2). The role played by *common language* is confirmed when we control for the *same country* dummy (Appendix II: A15). Language could affect all home-sovereign relations, not only those in which both countries share a common language. Kolo's (2012) *cultural distance (language)* variable provides a more nuanced measure of linguistic differences. The literature on cultural distance suggests that language dissimilarities reduce the amount of collected information and could thus lead to a higher predicted likelihood of default. Moreover, the less familiar a language, the less trust might be put into the message it delivers. The results in Table II.3 support these ideas: the coefficient on *cultural distance (language)* is negative and significant at the one-percent level in all specifications. The size of the effect is not negligible. Consider, for example, the linguistic difference between the United States and China (99), which is 20 percentage points higher than the difference between the United States and the Netherlands (79). A 20-point cultural distance in terms of language relates to a rating that is 0.54 points lower on average in the GFC period (column 2). This result is in line with findings of a cultural home bias for bank lending, investment decisions and trade patterns (Grinblatt and Keloharju 2001; Guiso et al. 2009; Giannetti and Yafeh 2012).

C*ultural distance (ethno-racial)* between home and sovereign, however, is not related to lower ratings. The respective coefficient is insignificant in all specifications. As there is no empirical evidence of discriminatory behavior of rating agencies based on race or ethnicity, the cultural home bias instead appears to be a function of the availability of information and risk perceptions. Familiarity in terms of language also goes along with a general cultural familiarity, which can be conducive to bilateral trust and better cooperation in the rating process. In Section 5.3, we will further explore the transmission channel behind the "cultural home bias."

Next, we include the statistically significant proxies of economic and cultural ties from our main specification (Table II.3, column 1) in a single equation to test whether the individual effects still remain significant if jointly included in one regression. Economic and cultural ties are intertwined: countries that are culturally close to the home country are on average those with which the home country entertains close economic relations (Guiso et al. 2009). However, even after adding the two language variables in columns 1 and 2 of Table II.4, respectively, the coefficients on *bank exposure* remain stable and statistically significant at conventional levels. Also, both *common language* and *cultural distance (language)* keep their signs and remain statistically significant. We conclude that the effect of economic ties is not (entirely) driven by cultural distance between the home country and the sovereign as the effect of the former remains significant when we control for the latter. Next, we explore if both language variables are substitutes for each other. As *bank exposure* is not available for China, Cyprus, the home country itself and years prior to 2005, column 3 excludes this measure and focuses on the two language variables. Conditioning on each other lowers the coefficients in absolute terms, but both language variables stay significant at conventional levels. Column 4 explores the channels that could explain the positive *same country* relationship. Conditioning on cultural distance, the coefficient on *same country* decreases in size and becomes insignificant, while both language variables remain statistically significant. This suggests that cultural proximity is the main transmission channel that leads to the differential treatment of the home country.[74]

[74] As one seminar participant suggested, cultural proximity and common language could be closely related to a sovereign's legal origin. For example, French-speaking countries are more likely to share their legal origin with France. Appendix II: A16 replicates columns 1 and 2 of Table 4 including a dummy variable that takes a value of one if the home

Our regressions so far include time- and agency-fixed effects to exploit cross-country variation since ratings are rather sticky and they show little variation for some countries. For those countries, a potential home bias could be reflected mostly in their initial rating level; and not be fully captured in an empirical setting where identification comes only from variation over time. Still, our results are robust when we run regressions according to Equations (4) and (5), respectively. While column 1 of Table II.5 displays the main results from Table II.3 for the reader's convenience, columns 2 and 3 show the results with the additional controls. As can be seen, the findings for *same country*, *bank exposure* and *cultural distance (language)* are largely robust to the inclusion of additional fixed effects. *Same country* becomes significant at the one-percent level when estimating the models in Equation (4) and remains so even in equation (5) where we control for all country-specific time-invariant and time-variant unobserved factors. *Bank exposure* reaches statistical significance at the five-percent level when introducing agency-sovereign-pair-fixed effects and is only marginally insignificant with agency-sovereign-pair-year-fixed effects (p-value: 0.105). While *common language* loses its statistical significance, *export interests*, *geopolitical alignment (UN)*, *US military interests (aid)*, and *cultural distance (ethno-racial)* remain insignificant.[75] This shows that our results are not driven by factors that are time-invariant in our sample such as the advantage that the United States enjoys with the US dollar being the world's reserve currency.

Taken together, we can reject the null hypothesis of unbiased sovereign ratings. Agencies seem to assign relatively better ratings to their home country and to countries that are economically aligned with it in terms of bank exposure. There is strong support for a role of cultural distance in terms of common or similar language, but not of ethno-racial differences. There is no robust evidence for a role of geopolitical ties.

---

country and rated country share the same legal origin (data from Treisman 2007). Our results are largely unaffected. The coefficients on same legal origin do not reach statistical significance with the exception of the regression with *US military interests (aid)*, which covers US agencies only.

[75] The only exception is *export interests* when using agency-sovereign-pair-year-fixed effects. The corresponding coefficient is negative and statistically significant at the ten-percent level.

5.2 Agency-specific Regressions

By pooling all agencies in one regression, we have implicitly assumed that each agency weighs all sovereign-specific factors in the same way as we were estimating a single coefficient per explanatory variable. We now relax this assumption and run individual regressions for each of the nine rating agencies under analysis. This allows us to investigate differences between the nine agencies under study.

For each agency's home country, Figure 2 contrasts the actual rating and the predicted rating for the home country based on how each agency weighs the economic and political fundamentals of sovereigns based on the least squares estimations. In other words, we compare the actual ratings (solid line) with the ratings that should be assigned to the home country if it were not the home country (dotted line). The figure highlights several interesting results. First, with respect to all US agencies since the early 2000s recession, the predicted values for the United States are lower than the actual rating. While Fitch and Moody's still deviate from their lower predicted value for the U.S. by more than one point (as of June 2013), S&P came closer to its predicted value after the agency downgraded its home country in August 2011. Second, while both JCR and R&I assign a high rating to their home country Japan, the dotted line indicates that only R&I seems to apply different criteria to Japan compared to other countries. Third, Dagong's predicted rating for China is increasingly deviating from the assigned "AAA" rating as China's fundamentals have deteriorated since 2011.

Table II.6 shows the coefficients of our variables of interest for each agency individually.[76] It is reassuring that the coefficients on *same country* are within a reasonable range between 0.252 (Moody's) and 2.483 (Dagong). However, the only exception, Feri, highlights one potential problem of OLS. Feri assigns the highest possible "AAA" (21) rating to Germany throughout the sample period. However, the negative *same country* coefficient suggests that the predicted rating based on the treatment of the sovereign-specific factors of other sovereigns is above 21. As can be seen in Figure 2, this also occurs for Fitch, Moody's, S&P and, to a lesser degree, for JCR. This

[76] The interested reader will find the results of agency-specific regressions excluding the variables of interest in Appendix II: A17. The R-squared values and number of observations corresponding to the regressions of Tables 6 and 7 are displayed in Appendix II: A18.

implies that the coefficient on *same country* will be biased downwards in these cases. Being aware of this caveat, we find that only CI, Dagong, Fitch and R&I provide a significantly higher rating to the country where their headquarters is based, while the coefficient for S&P is marginally significant (p-value: 0.100). By re-running the agency-specific regressions using ordered probit, we account for the bounded nature of the dependent variable. Table II.7 shows that *same country* also becomes statistically significant at the one-percent level for Feri, Fitch, JCR, Moody's, and S&P, i.e., those agencies for which we argued above that the OLS coefficient could be negatively biased. This suggests that this significant positive relationship is a broad phenomenon and not restricted to single agencies.[77]

We now turn to our measures of economic interests between home and sovereign (see again Tables II.6 and II.7). While *export interests* did not gain significance in the pooled regressions of Table 3, the agency-specific regressions reveal a more nuanced picture. The respective coefficient becomes significantly positive at conventional levels for Moody's and S&P according to both tables, for JCR according to Table II.6 (OLS) and for Feri according to Table II.7 (ordered probit). The unexpected statistically significant negative coefficient for CI disappears when we omit Greece, which is by far Cyprus' largest export market and could be regarded as an outlier given Greece's economic turmoil during the sample period. The evidence on *bank exposure* is much more conclusive and supports the results from the pooled regressions. For four (OLS) and six (ordered probit) agencies, the corresponding coefficient is found to be positive and statistically significant at conventional levels. The largest effect exists for Feri, where a ten-percent increase in the share of overall exposure to the rated country leads to a 1.5-point increase in its rating (column 4).

Geopolitical ties seem to be largely irrelevant for the rating decisions of most agencies. With regard to *geopolitical alignment (UN)*, the coefficient is insignificant in most specifications and even significantly negative for Feri and JCR (and R&I with ordered probit). The one notable exception is Dagong where the coefficient is positive and statistically significant at the one-percent level with both OLS and ordered probit. It seems that Dagong assigns higher ratings to those countries that are politically more aligned with the Chinese government. This effect is sizable and robust in terms

[77] Note that the maximum likelihood procedure did not converge for DBRS, the smallest agency in our sample in terms of number of rated countries (25).

of size and significance to the inclusion of the *same country* dummy (not displayed). A country receives a rating that is one full point higher if its political alignment with China increases by 17 percentage points. This is approximately the difference between Brazil's (89 percent) and Turkey's (72 percent) voting alignment with China in the UN General Assembly in 2012. While the coefficient on *military interests (aid)* does not reach statistical significance at conventional levels in the OLS specifications, it becomes statistically significantly positive for Fitch and Moody's when using ordered probit. Thus there is no robust evidence that US agencies assign higher ratings to countries that are of military interest to the US government. Overall, there is no robust evidence that geopolitical ties between home and sovereign have an impact on rating decisions for all agencies except Dagong.

Again, the results in Tables II.6 and II.7 highlight the importance of cultural relatedness. The coefficients on *common language* are positive in all cases but one (CI), and the effect of *cultural distance (language)* is negative in all cases. For eight out of nine agencies at least one of the two variables is statistically significant at conventional levels in Tables II.6 and II.7. CI and Dagong are the only agencies for which we find no evidence that they assign higher ratings to countries that share a *common language* with their home country. The more nuanced *cultural distance (language)* reaches statistical significance at conventional levels for all agencies except CI according to the results in at least one of the two tables. These findings suggest that the cultural home bias is a general phenomenon and not restricted to individual rating agencies.

Finally, we consider an alternative definition of home country. Rather than defining the home country based on the location of the agency's headquarters, we apply the ownership definition. Thus, CI is coded as "Kuwait" and Fitch is coded as "France." As can be seen from column 10 in Tables II.6 and II.7, Kuwait does not receive a relatively better rating from CI as in the case of Cyprus, the location of CI's headquarters. However, CI assigns significantly higher ratings to Arab-speaking countries and countries that are linguistically or ethno-racially closer to Kuwait. This might be related to the large share of employees of Arabic origin. For Fitch, we find a positive coefficient on *same country* when we apply the ownership definition instead of the location definition (column 11). According to the OLS results, Fitch's rating of France is one point better than what would be expected by the agency's treatment of other countries. The variables for cultural distance are all insignificant in contrast to the results with the U.S. as the home country in column 5. As it does not seem that Fimalac's majority shareholdings are reflected in a particularly

high number of French staff, this is not surprising. In summary, these results can be taken as evidence that it is not only the location of an agency that matters for rating outcomes but also the ownership structure.

5.3 Exploration of Transmission Channels

In this subsection, we examine the transmission channels of cultural distance in more detail. The insignificant coefficients for *cultural distance (ethno-racial)* suggest that discrimination based on race and ethnicity is unrelated to rating outcomes. The two remaining theoretical explanations for the role of culture, namely superior information and differences in risk perception, are both consistent with the negative effect of *cultural distance (language)* on sovereign ratings. If information were the main transmission channel, we would expect that the bias would be alleviated by the existence of a foreign office in the rated country. Giannetti and Yafeh (2012), for example, find that the cultural bias in bank lending is mitigated when banks have a subsidiary in the foreign country. To test for this transmission channel, we code a dummy variable *office* that takes a value of one if an agency has an office in the rated country. Only the big U.S.-based agencies have foreign offices on a global scale (Fitch: 34, Moody's: 24, S&P: 24), with Feri having a total of 6 five agencies. We thus restrict our analysis to these four agencies. Table II.8 shows that both *office* as well as the interaction between the dummy variable and *cultural distance (language)* are not statistically significant at conventional levels for any of the agencies. The coefficient on *cultural distance (language)* remains negative and statistically significant at conventional levels in all specifications. The finding that the local presence of agency staff does not mitigate the cultural home bias suggests that the availability of information is not the main driver behind cultural distance's effect.

The other explanation for the cultural bias is a more pessimistic perception of risks in culturally less similar countries. To test explicitly for the existence of such a link, we analyze whether bilateral trust mitigates the effect of cultural distance. Data on bilateral trust are only available for a limited set of European countries (see, for example, Guiso et al. 2009).[78] Hence, we are only able

[78] In analogy to Guiso et al. (2009), we use the trust level of a representative German citizen towards a randomly selected individual of the rated country. This time-invariant trust level is constructed by coding the answers to the following Eurobarometer question on a 4-point scale: "I would like to ask you a question about how much trust you have in people from various countries. For each, please tell me whether you have a lot of trust, some trust, not very much trust, or no trust at all." We are grateful to Shu Yu for sharing the data with us. See Yu et al. (2015) for details.

to test this channel for the German agency Feri. As expected, the results in Table II.9 show a highly significant positive effect of bilateral trust on rating levels. Moreover, as expected there is a significant positive interaction effect between *cultural distance (language)* and *bilateral trust* of Germans in the rated country. The marginal effects graphically displayed in Appendix A19 show that cultural distance matters more when trust in individuals from the rated country is low. This is preliminary evidence that the effect of cultural distance on sovereign ratings is (partly) caused by a more pessimistic perception of risks when bilateral trust is low.

Finally, we run quantile regressions to learn more about the mechanisms behind the effect of cultural distance. Quantile regressions serve both as a test for outliers by showing the size and direction of the effect of culture at different quantiles of the rating distribution as well as allowing us to test whether the effect of cultural distance on ratings is larger for countries at the lower end of the distribution. In the literature that explains home bias with information asymmetry, agents receive public and private information signals, but the private signal is less precise for foreign agents (see, for example, Gehrig 1983; Brennan and Cao 1997). Financial information for less developed countries (lower ratings, hence at lower quantiles), is often of poor quality and low credibility (Ahearne et al. 2004), thus the relative importance of private information is higher. In these cases, differences in risk perceptions should become more important. As expected, the negative effect of *cultural distance (language)* on rating outcomes is larger for countries at the lower end of the rating distribution (see Table II.10). It increases in absolute terms from -0.011 at the .8 quantile to -0.024 at the .2 quantile. The effect of *cultural distance (language)* is consistently negative and significant at the one-percent level across the rating distribution. Comparing these findings with results for the GFC sample, we find the largest difference in the size of the effect at the lower end of the rating distribution. At the .2 and .4 quantile, the negative coefficient nearly doubles in the GFC period, while at the upper end the differences are much smaller compared to the full sample. These results suggest that the differences in risk perceptions rooted in culture matter more when information is sparse and uncertainty is higher.

5.4 Further Robustness Checks

In this subsection, we test the robustness of our results with respect to further alterations in the empirical strategy. First, we control for selection of countries into the sample of rated countries. For example, a sovereign could be more likely to request a rating from an agency from which it expects a more favorable rating. To control for a possible selection bias, we rerun our model from Table II.3 (column 1) as a Heckman selection model. However, it is difficult to find a suitable exclusion variable. When they predict the probability that Moody's and S&P assign a rating to a particular sovereign, Beaulieu et al. (2012) use decade-fixed effects and a rated country's exports to the United States as exclusion variables. While the former is meant to capture a positive trend in the total number of rated countries, the latter is intended as a proxy for "a country's friendliness with major western powers" (Beaulieu et al. 2012: 721). However, there are reasons to doubt that either of these two variables satisfies the exclusion restriction. While decade-fixed effects could reflect the global economic situation and thus directly impact rating levels, home-country export patterns might be related to rating outcomes, as our paper argues. Note also that we find *export interests* to exhibit a significant positive relationship with rating outcomes in our regressions for JCR, Moody's and S&P (Table II.6, columns 6, 7 and 9). Instead, we choose to start by estimating a Heckman model without exclusion variables. We then use the number of sovereigns that are rated by the respective agency in the previous period, the number of the big three agencies that rate a sovereign in the previous period, or both as exclusion variables. Arguably, a sovereign is more likely to be rated by an agency if that agency covers a larger set of countries and if the market-dominating agencies rate a particular country. At the same time, it is difficult to come up with an explanation as to why these two measures of country coverage should directly affect rating levels. The results support our earlier conclusions when we do not account for selection, the main differences being that *bank exposure* now reaches the one-percent level of significance across all specifications and that *US military interests (aid)* becomes statistically significant at the five-percent level (see Appendix II: A20 for full regression results).

Second, we address the potential role of the business model of rating agencies, specifically conflicts of interest that could arise from an "issuer-pays" model. Afonso (2003: 60) mentions the possibility that agencies attempt "to gain market share by giving some countries a notch more than the competitors." Similarly, White (2010: 215) suggests that a "rating agency might shade its rating upward so as to keep the issuer happy and forestall the issuer's taking its rating business to a

different rating agency." One might thus suspect that countries that pay for their ratings would be treated more favorably. To our knowledge, S&P is the only agency that publishes information on which countries receive unsolicited ratings. These countries are Argentina, Australia, Belgium, Cambodia, France, India, Italy, Japan, the Netherlands, Singapore, Switzerland, Taiwan, Turkey, the United Kingdom and the United States (as of June 2013).[79] As can be seen from Appendix II: A21, accounting for a dummy that indicates an unsolicited rating and its interaction with our variables of interest does not qualitatively alter our results for S&P with regard to *common language* and *cultural distance* (compare Table II.6 column 9). Interestingly, *bank exposure* only becomes statistically significant for countries with unsolicited ratings (p-value: 0.001).

Finally, we discuss whether our results could be affected by endogeneity. Among the three variables of interest that robustly indicate a home bias in sovereign ratings (*same country*, *bank exposure*, *cultural distance (language)*), the most obvious candidate is *bank exposure*. Banks might respond to rating changes and lend more to countries with improved ratings, reversing the causal direction. However, this concern is mitigated by the use of lagged values of bank exposure in our estimations and by the fact that the results hold in the country-pair-fixed effects specification, which relies only on variation over time to identify the parameters (see again Table II.5). *Bank exposure* might also exhibit a spurious positive correlation with rating levels if bank analysts are prone to a similar home bias as rating analysts. This could be the case as the previous literature shows that professional investors are also more likely to invest in companies that are culturally closer (e.g., Grinblatt and Keloharju 2001). However, our findings do not seem to be caused by such a spurious correlation: the results for bank exposure hold when controlling for *common language* and *cultural distance (language)* (Table II.4) and they are also confirmed when applying country-pair-fixed effects (Table II.5). With respect to *same country* and *cultural distance*, it is difficult to raise similar concerns. The causal direction is clear for cultural distance as culture is a deeply rooted phenomenon and should not respond to short-term rating changes.

[79] See S&P website available at http://www.standardandpoors.com/ratings/sovereigns/ratings-list/en/us?sectorName=null&subSectorCode= (accessed June 2013).

6. Conclusions

Observers frequently perceive sovereign ratings as being biased and doubt that they accurately reflect the economic and political fundamentals of the rated countries. Many of these concerns reflect the idea that the home country of credit rating agencies has a systematic influence on rating outcomes. The aim of this study is to provide an objective and systematic empirical assessment to test these claims. For this matter, we refer to a home bias in sovereign ratings if a rating agency assigns relatively better ratings to its home country and to countries with close economic, political and cultural ties to its home. Building on the respective literature on political economy influences in economic assessments and on cultural biases in financial decision-making, we discuss potential reasons why such a bias could exist.

To test whether there is empirical support for different kinds of home biases, we collect rating data from nine agencies based in six countries. While most of the variation in ratings is explained by the economic and political fundamentals of rated countries, our results also suggest that sovereign ratings in fact exhibit biases. We find that the average agency assigns a rating to its home country that is almost one point higher than what seems justified by how it assesses other sovereigns. While there is no robust empirical support that geopolitical ties between home and sovereign play a significant role in rating outcomes, there is evidence that agencies on average assign relatively higher ratings to countries to which their home-country's banks have a larger risk exposure. Moreover, we find that countries that are culturally closer receive a better treatment: the larger the linguistic differences between home and sovereign, i.e., the more unfamiliar a language, the lower the assigned rating on average. The effects of bank exposure and cultural distance appear to be largely independent of one another. Taken together, it appears that the home bias is mainly the result of economic interests and cultural proximity, rather than being geopolitically motivated. Moreover, cultural proximity seems to be the main transmission channel that causes the differential treatment of the home country itself. These results are robust to the choice of estimation methods, the inclusion of country-pair- or rated-country-fixed effects, and selection into the sample of rated countries. The individual analysis of the nine agencies suggests that the home bias is neither only restricted to US agencies nor to the smaller agencies, but rather appears to be a generalizable phenomenon.

We hypothesize that a cultural bias could arise from a lack of information, differences in risk perceptions or simply from discrimination. We find no support for discrimination based on ethno-racial differences between home and sovereign. If the differences were solely due to a lack of information, the existence of an office in a rated country should alleviate the bias. However, when we interact the existence of an office with cultural distance, the bias is not mitigated. Thus, the most plausible explanation appears to be that cultural distance is related to less trust and more pessimistic risk perceptions and, thereby, lower ratings. We present preliminary evidence that supports this explanation.

These results should not be taken as evidence that rating agencies do not fulfil an economically relevant and potentially efficiency-enhancing role. Supposedly objective sovereign-specific economic and political fundamentals explain most of the variation in sovereign ratings. Still, the economic significance of the home bias is not negligible. Biases of one rating point can make a big difference for the degree to which a country has access to international capital markets. This holds at least as long as many large investors are forced by regulations to depend on credit ratings for their portfolio choices. That said, our finding of a cultural home bias does not necessarily reflect irrational behavior on behalf of the agencies. Their judgments are based on imperfect information provided by the rated countries as well as third parties. It can be rational to rely on heuristics such as bilateral trust to evaluate this information, which can lead to a lower perceived likelihood that a more familiar state defaults on its debt.

There are important policy implications from our results. Regulation should aim at fostering competition and decrease the reliance on a few big agencies. While transparency can be beneficial, overly rigid regulatory frameworks could prevent rating agencies from adequately and quickly adjusting their methodologies and models to new circumstances and thus lower market efficiency. Attempts to limit the maximum number of rating updates to three times a year for unsolicited ratings (as proposed by the European Commission) would particularly affect smaller agencies who issue fewer solicited ratings. Hence this regulatory measure could stifle competition. A more promising approach that is in line with our results would be to explicitly embrace the plurality of methods and opinions. In cases where economic arguments support the use of external ratings, regulation should require ratings by several agencies, ideally from different countries and cultural backgrounds. This would provide a more comprehensive risk assessment and automatically lead to a more diverse and competitive rating agency landscape.

Table II.1: Overview on major credit rating agencies

Agency	Headquarters	Founded	Sov. rating since	Country offices	Staff	Ownership as of December 2012	Ownership history	Number of rated sovereigns by the end of	
								2000	2012
CI	Limassol, Cyprus	1982	2002	3	~20	100% privately owned by Afaf Adham, Amin Diab, Zafer Diab and the Gulf Master International Co. (privately-owned Kuwaiti Company); none of the shareholders has majority or voting control	No publicly known changes	-	37
Dagong	Beijing, China	1994	2010	3	~600	100% privately owned; major shareholder Guan Jianzhong (former Chinese government official)	Privately owned since foundation; Guan Jianzhong became major shareholder and president in 1998	-	72
DBRS	Toronto, Canada	1976	1998	3	~200	100% privately owned by Walter Schroeder since 1976	No publicly known changes	1	25
Feri	Bad Homburg, Germany	1987	1999	5	~50	100% owned by MLP AG (publicly-traded German company) since 2011; major shareholders of MLP are Manfred Lautenschläger (23.38%), Harris Associates (9.82%), Swiss Life (9.9%), HDI Talanx AG (9.89%), Allianz SE (6.27%)	30% owned by Harald Quandt Holding GmbH and 70% owned by Feri partners until 2006; MLP AG acquired 56,6% including the 30% stake of the Quandt Holding GmbH in 2006	55	56
Fitch	New York, USA; London, UK	1913	1994	34	~2,000	50% Hearst Corporation (family-owned US-based multinational mass media group) and 50% Fimalac (French holding company, major owner Marc Ladreit de Lacharrière)	Part of Fitch Group (100% owned by Fimalac) until 2006; 20% were sold to the Hearst Corporation in 2006, additional 20% in 2009 and another 10% in 2012	69	101
JCR	Tokyo, Japan	1985	1998	1	~90	Stock company, largest shareholders: Jiji Press, Ltd. (19.71%), JCR employees' stock ownership associations (6.51%), K.K. Kyodo News (5.93%), Sumitomo Life Insurance Company (2.68%), Meiji Yasuda Life Insurance Company (2.68%)	No publicly known changes	21	33
Moody's	New York, USA	1909	1918	24	~6,800	Publicly traded since 2000; institutional ownership: 95.34%; top 5 institutional shareholders: Berkshire Hathaway Inc. (13.4%), Capital World Investors (8.1%), Vanguard Group Inc. (6.2%), Bank of New York Mellon Corp (4%), Massachusetts Financial Services Co. (3.8%); further shareholders: BlackRock, Morgan Stanley, State Street, Northern Trust Corp., T. Rowe Price Associates	Owned by US company Dun & Bradstreet from 1961-2000	82	113
R&I	Tokyo, Japan	1998	1998	2	~250	Stock company and part of Nikkei Group; largest shareholders: Nikkei, Inc. (42.72%), Nikkei Business Publications, Inc. (13.41%), Quick Corp. (8.24%), The Bank of Tokyo-Mitsubishi UFJ, Ltd. (4.91%), Sumitomo Mitsui Banking Corp. (4.60%)	Established through the merger between Japan Bond Research Institute and Nippon Investors Service, Inc.	42	44
S&P	New York, USA	1860	1922	24	~5,000	100% owned by McGraw Hill Companies, Inc., since 1966; major shareholders of the publicly-traded McGraw Hill Companies: Capital World Investors, Vanguard Group, State Street Corp., Oppenheimer Funds Inc., Morgan Stanley, Inc.; further shareholders: BlackRock, Bank of New York Mellon Corp., Northern Trust Corp., T. Rowe Price Associates	In 1941, merger of Poor's Publishing (founded 1860) and Standard Statistics (founded in 1906)	87	125

Sources: Personal communication with Moody's Client Services (28 February 2013, 4 March 2013, 22 March 2013, 25 March 2013), Feri EuroRating Services AG (14 March 2013, 21 March 2013), Dagong Global Credit Rating (5 April 2013, 3 June 2013), internet research (see Appendix II: A22 for a detailed list of sources), and Gaillard (2012).

Table II.2: Sovereign ratings of home countries (as of June 2013)

	Canada	China	Cyprus	Germany	France	Japan	Kuwait	United States
Home agencies	AAA (21)	AAA (21)	B (7)	AAA (21)	AAA (21)	AAA (20.5)	AA- (18)	AAA (20.7)
Other agencies	AAA (20.7)	AA- (18)	CCC (4.4)	AAA (20.9)	AA+ (19.9)	A+ (17.4)	AA (19)	AA+ (19.8)

Notes: Each cell displays the three-letter rating. In cases where more than one (home or other) agency assigns a rating to a particular country, the table displays the average rating from either all home agencies or all other agencies.

Table II.3: Home biases in sovereign ratings (all agencies pooled)

	(1)	(2)	(3)	(4)
	Least squares		Ordered probit	
	Full sample	GFC sample	Full sample	GFC sample
(a) Same country	0.950**	1.704***	1.591***	1.545***
	[0.018]	[0.001]	[0.000]	[0.000]
(b) Export interests	0.017	0.030	0.017	0.027
	[0.345]	[0.142]	[0.144]	[0.142]
(c) Bank exposure	0.038*	0.045*	0.076***	0.086***
	[0.054]	[0.070]	[0.005]	[0.000]
(d) Geopolitical alignment (UN)	0.005	0.013	0.004	0.009***
	[0.518]	[0.183]	[0.183]	[0.002]
(e) US military interests (aid)	0.034*	0.116***	0.026*	0.085***
	[0.082]	[0.001]	[0.076]	[0.005]
(f) Common language	0.742***	0.810***	0.452***	0.444**
	[0.000]	[0.001]	[0.001]	[0.022]
(g) Cultural distance (language)	-0.021***	-0.027***	-0.017***	-0.018***
	[0.000]	[0.000]	[0.000]	[0.000]
(h) Cultural distance (ethno-racial)	0.001	0.003	0.001	0.002
	[0.718]	[0.472]	[0.443]	[0.141]

Notes: The dependent variable is a country's sovereign rating on a 21-point scale. Each cell refers to a separate regression. The table displays only the coefficients on the respective variable of interest of each regression. All regressions contain the control variables as specified in Appendix II: A12, time- and agency-fixed effects. The full sample contains data from January 1990 to June 2013 (columns 1 and 3). The GFC sample runs from September 2008 to June 2013 (columns 2 and 4). Appendix II: A13 shows the results of the control variables for the models in column 1. Appendix II: A14 displays the adjusted R-squared values and the number of observation of each regression. Standard errors are clustered at both the agency-time and the sovereign level. ***,**,* indicate significance at the one-percent, five-percent or ten-percent level. P-values are displayed in brackets.

Table II.4: Home biases in sovereign ratings (OLS, all agencies pooled, robustness checks)

	(1)	(2)	(3)	(4)
Bank exposure	0.029*	0.030*		
	[0.075]	[0.094]		
Common language	0.753**		0.473*	0.476*
	[0.012]		[0.069]	[0.068]
Cultural distance (language)		-0.022**	-0.014**	-0.015**
		[0.012]	[0.018]	[0.021]
Same country				-0.086
				[0.817]
Agency-fixed effects	Yes	Yes	Yes	Yes
Time-fixed effects	Yes	Yes	Yes	Yes
Adjusted R-squared	0.87	0.87	0.87	0.87
Observations	37743	37743	74701	74701

Notes: The dependent variable is a country's sovereign rating on a 21-point scale. The table displays only the coefficients on the respective variables of interest of each regression. All regressions contain the control variables as specified in Table II.3, agency and time-fixed effects. Data range from January 1990 to June 2013. Standard errors are clustered at both the agency-time and the sovereign level. ***,**,* indicate significance at the one-percent, five-percent or ten-percent level. P-values are displayed in brackets.

Table II.5: Home biases in sovereign ratings (OLS, all agencies pooled, additional fixed effects)

	(1) Equation 3		(2) Equation 4		(3) Equation 5	
(a) Same country	0.950**	Agency FE	0.681***	Agency FE and sovereign FE	0.699***	Agency FE and Sovereign-year FE
	[0.022]		[0.004]		[0.001]	
(b) Export interests	0.017	Agency FE	-0.095	Agency-sovereign pair FE	-0.216*	Agency-sovereign pair-year FE
	[0.363]		[0.269]		[0.071]	
(c) Bank exposure	0.038*	Agency FE	0.112**	Agency-sovereign pair FE	0.023	Agency-sovereign pair-year FE
	[0.077]		[0.024]		[0.105]	
(d) Political alignment (UN)	0.005	Agency FE	-0.003	Agency-sovereign pair FE	0.006	Agency-sovereign pair-year FE
	[0.358]		[0.849]		[0.621]	
(e) US military interests (aid)	0.034	Agency FE	0.03	Agency-sovereign pair FE	0.006	Agency-sovereign pair-year FE
	[0.192]		[0.325]		[0.621]	
(f) Common language	0.742***	Agency FE	0.101	Agency FE and sovereign FE	0.150	Agency FE and Sovereign-year FE
	[0.001]		[0.689]		[0.569]	
(g) Cultural distance (language)	-0.021***	Agency FE	-0.008**	Agency FE and sovereign FE	-0.011***	Agency FE and Sovereign-year FE
	[0.000]		[0.011]		[0.001]	
(h) Cultural distance (ethno-racial)	0.001	Agency FE	0.003	Agency FE and sovereign FE	0.002	Agency FE and Sovereign-year FE
	[0.589]		[0.192]		[0.257]	

Notes: The dependent variable is a country's sovereign rating on a 21-point scale. Each cell refers to a separate regression. The table displays only the coefficients on the respective variable of interest of each regression. All regressions contain the control variables as specified in Appendix II: A12, time-fixed effects as well as the fixed effects specified in the table. Column 1 is identical with column 1 of Table II.3 to facilitate reading. Data range from January 1990 to June 2013. Standard errors are clustered at both the agency-time and the sovereign level. ***,**,* indicate significance at the one-percent, five-percent or ten-percent level. P-values are displayed in brackets.

Table II.6: Home biases in sovereign ratings (OLS, by agency)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	CI	Dagong	DBRS	Feri	Fitch	JCR	Moody's	R&I	S&P	CI	Fitch
	CYP	CHN	CAN	DEU	USA	JPN	USA	JPN	USA	KWT	FRA
(a) Same country	1.950*	2.483*	0.969	-0.223	0.876*	0.590	0.252	1.985**	0.759'	0.561	1.063***
	[0.053]	[0.095]	[0.178]	[0.576]	[0.058]	[0.574]	[0.620]	[0.014]	[0.100]	[0.520]	[0.000]
(b) Export interests	-0.387***	-0.113	0.014	0.098	0.03	0.081*	0.041*	-0.001	0.047*	0.61	0.076
	[0.000]	[0.200]	[0.366]	[0.189]	[0.272]	[0.071]	[0.051]	[0.982]	[0.057]	[0.344]	[0.122]
(c) Bank exposure			-0.021	0.150***	0.107**	0.060***	0.062	0.039	0.135***		0.052
			[0.358]	[0.000]	[0.013]	[0.009]	[0.129]	[0.184]	[0.001]		[0.222]
(d) Geopolitical alignment (UN)	-0.016	0.058***	0.024	-0.070***	0.017	-0.069***	0.004	-0.038	-0.001	-0.001	-0.017
	[0.786]	[0.007]	[0.535]	[0.000]	[0.145]	[0.001]	[0.762]	[0.154]	[0.946]	[0.972]	[0.345]
(e) US military interests (aid)					0.036		0.034		0.033		
					[0.141]		[0.122]		[0.271]		
(f) Common language	-1.494	1.039	0.737*	0.752	0.880***		0.869***		0.810***	2.611**	0.028
	[0.152]	[0.334]	[0.081]	[0.109]	[0.001]		[0.005]		[0.002]	[0.015]	[0.934]
(g) Cultural distance (language)	-0.002	-0.035*	-0.072***	-0.020**	-0.033***	-0.002	-0.024**	-0.015	-0.028***	-0.033**	0.003
	[0.931]	[0.050]	[0.001]	[0.025]	[0.004]	[0.857]	[0.023]	[0.118]	[0.003]	[0.016]	[0.631]
(h) Cultural distance (ethno-racial)	-0.008	0.006	0.001	-0.004	-0.008	0.003	0.004	-0.007	0.003	-0.034**	0.002
	[0.335]	[0.521]	[0.952]	[0.525]	[0.430]	[0.670]	[0.711]	[0.489]	[0.797]	[0.015]	[0.699]

Notes: The dependent variable is a country's sovereign rating on a 21-point scale. Each cell refers to a separate regression. The table displays only the coefficients on the respective variable of interest of each regression. All regressions contain the control variables as specified in Appendix II: A12, time- and agency-fixed effects. Data range from January 1990 to June 2013. Columns (1) to (9) define the home country based on the agency's headquarters; columns (10) and (11) based on the ownership definition. Appendix II: A18 displays the adjusted R-squared values and the number of observation of each regression. Standard errors are clustered at both the sovereign and the time level. ***,**,* indicate significance at the one-percent, five-percent or ten-percent level. P-values are displayed in brackets.

Table II7: Home biases in sovereign ratings (ordered probit, by agency)

	(1) CI	(2) Dagong	(3) DBRS	(4) Feri	(5) Fitch	(6) JCR	(7) Moody's	(8) R&I	(9) S&P	(10) CI	(11) Fitch
	CYP	CHN	CAN	DEU	USA	JPN	USA	JPN	USA	KWT	FRA
(a) Same country	2.444***	6.949***	N/A	5.310***	5.493***	6.244***	5.375***	2.601***	1.46***	0.933	5.314***
	[0.008]	[0.000]		[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0000]	[0.190]	[0.000]
(b) Export interests	-0.711***	-0.082**	N/A	0.150**	0.015	0.177	0.032***	0.036	0.036**	0.853	0.071
	[0.000]	[0.046]		[0.015]	[0.252]	[0.136]	[0.006]	[0.425]	[0.022]	[0.133]	[0.142]
(c) Bank exposure			N/A	0.134***	0.117**	1.873***	0.089*	0.568***	0.164**		0.043
				[0.000]	[0.014]	[0.000]	[0.075]	[0.000]	[0.025]		[0.301]
(d) Geopolitical alignment (UN)	0.005	0.046***	N/A	-0.041***	0.015*	-0.153***	0.006	-0.121***	0.002	-0.002	-0.016
	[0.914]	[0.000]		[0.002]	[0.076]	[0.008]	[0.451]	[0.001]	[0.784]	[0.948]	[0.197]
(e) US military interests (aid)			N/A		0.034**		0.019*		0.028		
					[0.013]		[0.092]		[0.115]		
(f) Common language	-1.404	1.003	N/A	1.480***	0.507***		0.594***		0.466***	2.040*	0.041
	[0.233]	[0.164]		[0.000]	[0.003]		[0.002]		[0.009]	[0.075]	[0.882]
(g) Cultural distance (language)	-0.007	-0.049***	N/A	-0.035***	-0.021**	-0.023*	-0.021***	-0.019*	-0.020***	-0.029**	0.004
	[0.641]	[0.001]		[0.000]	[0.022]	[0.052]	[0.002]	[0.051]	[0.006]	[0.025]	[0.359]
(h) Cultural distance (ethno-racial)	-0.009	0.001	N/A	-0.007*	-0.004	-0.002	0.002	-0.004	0.002	-0.028*	0.002
	[0.119]	[0.754]		[0.091]	[0.477]	[0.746]	[0.737]	[0.600]	[0.735]	[0.050]	[0.632]

Notes: The dependent variable is a country's sovereign rating on a 21-point scale. Each cell refers to a separate regression. The table displays only the coefficients on the respective variable of interest of each regression. All regressions contain the control variables as specified in Appendix II: A12, time- and agency-fixed effects. Data range from January 1990 to June 2013. Columns (1) to (9) define the home country based on the agency's headquarters; columns (10) and (11) define it based on the ownership definition. For DBRS (column 3) the maximum likelihood procedure did not converge, most likely due to insufficient variation in the independent variables for each rating step. Standard errors are clustered at both the sovereign and the time level. ***,**,* indicate significance at the one-percent, five-percent or ten-percent level. P-values are displayed in brackets.

TableII.8: Do foreign offices decrease the effect of linguistic differences? (OLS)

	(1) Feri	(2) Fitch	(3) Moody's	(4) S&P
Cultural distance (language)	-0.026*	-0.036**	-0.027**	-0.024*
	[0.061]	[0.045]	[0.029]	[0.068]
Office in rated country	-0.643	-0.500	-0.593	0.695
	[0.670]	[0.748]	[0.668]	[0.574]
Cultural distance (language) * Office in rated country	-0.008	0.004	0.009	-0.008
	[0.638]	[0.823]	[0.588]	[0.568]
Adjusted R-squared	0.80	0.90	0.89	0.90
Observations	8730	14814	16374	18230

Notes: The dependent variable is a country's sovereign rating on a 21-point scale. Each column refers to a separate regression. The table displays only the coefficients on the respective variable of interest of each regression. All regressions contain the control variables as specified in Appendix II: A12, and time-fixed effects. Data range from January 1990 to June 2013. Standard errors are clustered at both the sovereign and the time level. ***,**,* indicate significance at the one-percent, five-percent or ten-percent level. P-values are displayed in brackets.

Table II.9: Interaction of cultural distance with bilateral trust (OLS, Feri only)

	(1)	(2)	(3)	(4)
Cultural distance (language)	-0.020**		-0.015	-0.456**
	[0.025]		[0.384]	[0.014]
Bilateral trust		5.320***	3.741*	-10.367
		[0.000]	[0.099]	[0.103]
Cultural distance (language) * Bilateral trust				0.148**
				[0.015]
Adjusted R-squared	0.81	0.84	0.85	0.85
Observations	5203	2334	2334	2334

Notes: The dependent variable is a country's sovereign rating on a 21-point scale. The regression contains the control variables as specified in Appendix II: A12 and time-fixed effects. The full sample contains data from June 1999 to June 2013. Standard errors are clustered at both the sovereign and the time level. ***,**,* indicate significance at the one-percent, five-percent or ten-percent level. P-values are displayed in brackets.

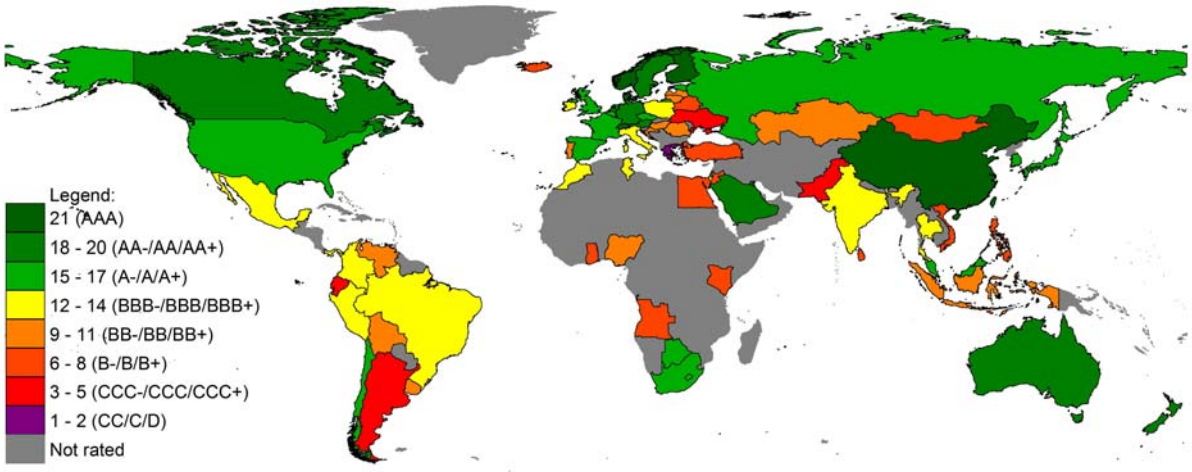
Table II.10: The effect of cultural distance on ratings at different quantiles (all agencies pooled)

		20%	40%	60%	80%
Cultural distance (language)	Full sample	-0.024***	-0.015***	-0.011***	-0.011**
		[0.002]	[0.001]	[0.002]	[0.014]
Cultural distance (language)	GFC sample	-0.044***	-0.028***	-0.019***	-0.016***
		[0.000]	[0.000]	[0.000]	[0.002]

Notes: The dependent variable is a country's sovereign rating on a 21-point scale. Each row refers to a separate regression. The table displays only the coefficients on the respective variable of interest of each regression. All regressions contain the control variables as specified in Appendix II: A12, time- and agency-fixed effects. The full sample contains data from January 1990 to June 2013. The GFC sample runs from September 2008 to June 2013. Standard errors are clustered at the sovereign level based on Parente and Santos Silva (2016). We use the demeaning procedure outlined in Canay (2011) to implement the fixed effects. ***,**,* indicate significance at the one-percent, five-percent or ten-percent level. P-values are displayed in brackets.

Figure II.1: Sovereign ratings by agency and country (as of 28 June 2013)

(a) Dagong (China)



(b) Moody's (USA)

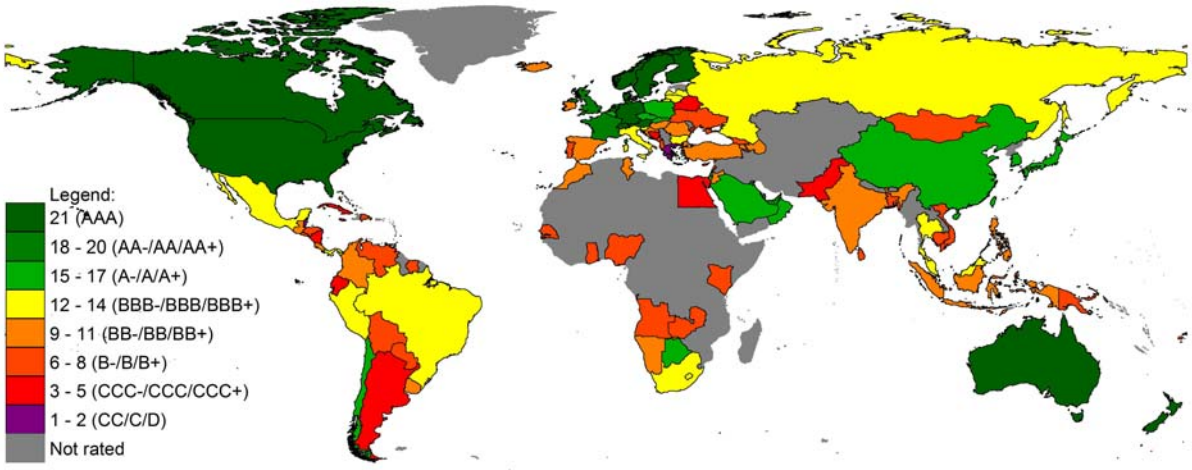
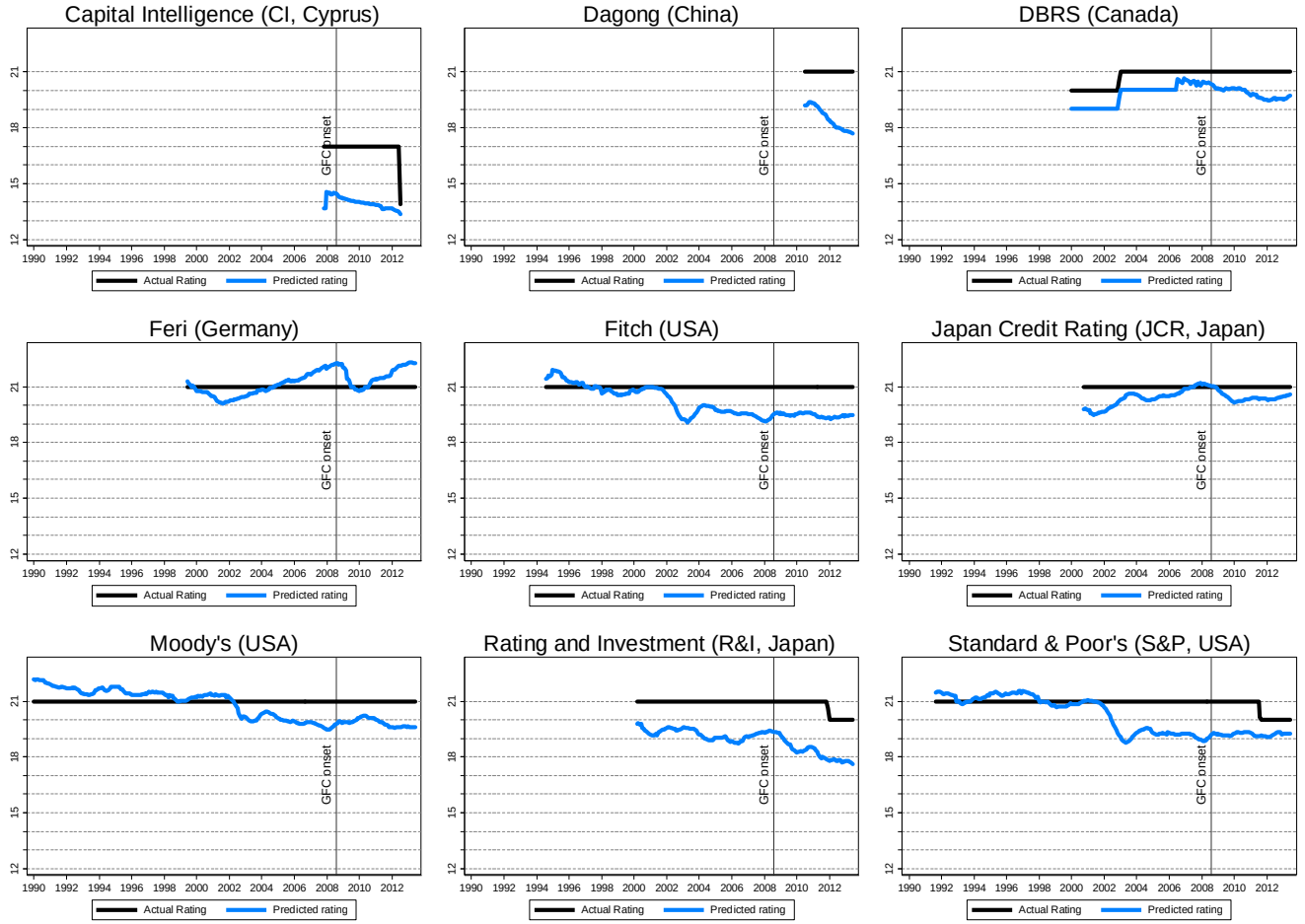


Figure II.2: Actual rating levels versus predicted values



Notes: Based on the specifications in Table II.6, this figure contrasts the actual rating and the predicted rating based on how each agency weighs the economic and political fundamentals of sovereigns. The solid lines depict the actual ratings, the dotted lines the ratings that would be assigned to the home country based only on the economic and political fundamentals.

III Crime, Incentives and Political Effort: A Model and Empirical Application for India

Coauthors:
T. Florian Kauffeldt (University of Heidelberg)
Krishna Chaitanya Vadlamannati (Norwegian University of Science and Technology)

Abstract:

The large share of politicians facing criminal accusations in India has sparked a public debate and an emerging literature that assesses its causes and effects. We develop a model of the incentives faced by members of parliament when deciding whether to engage in effort for their constituency to assess the effect of their having a criminal background on their decision. We use direct and clearly identifiable measures of effort in the 14th Lok Sabha over the 2004-2009 legislative period: attendance rates, parliamentary activity, and utilization rates of a local area development scheme. The findings suggest that criminal MPs exhibit on average about 5% lower attendance rates and lower utilization rates, but no difference in parliamentary activity. The results depend on the development level of the constituency, a proxy for rent-seeking possibilities and monitoring intensity, as well as on the measurement of criminal background. We use selection on observables, matching techniques, and treatment effect regressions to demonstrate why these negative relations should constitute an upper bound estimate for the causal effect of criminality and to show they are unlikely to be driven by selection on unobservables.

Keywords: India, Elections, Crime, Good and Bad Politicians, Development, Attendance and activity in parliament, Political economy

JEL classifications: D72, H11, I38

Aknowledgements: We thank Felix Arnold, Marta Curto, Axel Dreher, Vera Eichenauer, Andreas Fuchs, Philip Keefer, Marta Troya-Martinez, Christoph Vanberg and Thomas Wencker for comments, as well as Toke Aidt, Miriam Golden and Dewesh Tiwari for advice and sharing data with us. Thanks for comments and advice during conference presentations at the CSAE conference at Oxford University and an internal seminar at Heidelberg University. Important research assistance was provided by Katrin Mauch, Muhamed Kurtisi, Franziska Volk and Sven Kunze. All remaining mistakes are ours.

1 Introduction

India 2014: The world's largest democracy still features a parliament where roughly 34% of the recently elected members of the 16th Lok Sabha, the lower house of the Indian parliament, face pending criminal charges against them.[1] A significant share of those face serious criminal charges ranging from rape to murder.[2] The Indian and international media have reported on this issue, and it is widely believed that it poses a threat to the Indian democracy in general and the constituencies represented by those individuals specifically. But what are the specific economic and developmental consequences of parliamentarians with pending criminal charges holding public office?

While there is some anecdotal evidence that electoral constituencies represented by Members of Parliament (MPs) with serious criminal charges remain underdeveloped and such members tend to underperform in terms of their effort in parliament, systematic empirical evidence remains scant. Can criminal charges explain variation in parliamentarians' performance in parliament and the development of their electoral constituencies? To explore this question we develop a model of MP incentives in a principal agent approach where the voters takes the role of the principal. The existing literature has modeled the effect of electoral accountability on economic policy choices (Besley and Case, 1993), of compensation on policy outcomes (Besley, 2004), and of outside income opportunities on self-selection and behavior Gagliarducci et al. (2011). However, there is to the best of our knowledge no model that has approached the question of what drives MP effort in parliament, or more specifically for their constituency, once they are elected into office.

We connect to the emerging literature on electoral competition in the context of India. For instance, Aidt et al. (2015) investigate why parties field criminal candidates and Dutta and Gupta (2012) analyze competition between candidates that include criminals. In their seminal paper, Besley and Burgess (2002) model incumbents of different unobservable types who can exert effort to help voters in need. Since we focus on the relationship between incumbents and voters, our model is complementary to the first two

[1] See: http://timesofindia.indiatimes.com/news/Every-third-newly-elected-MP-has-criminal-background/articleshow/35306963.cms? The members of the 16th Lok Sabha were elected during the 2014 national elections held between 7th April and 12th May 2014. The 16th Lok Sabha commenced on 4th June 2014.

[2] Roughly 21% of the members face serious charges such as murder, rioting, theft, kidnapping, rape, etc., see: http://adrindia.org/research-and-reports/election-watch

papers and, in contrast to Besley and Burgess (2002), focuses particularly on the behavior and characteristics of criminal incumbents. In our model, MPs are aware that a higher effort level increases the probability of being reelected, but they lose utility from time allocated to political work. Thus, there is an immediate tradeoff between minimizing effort and the chance of being reelected. Other determinants of voting decisions matter as well. Using comparative statics with respect to the model parameters we demonstrate, for instance, that the effort of criminal MPs should be relatively higher when they contest in a more developed electoral district.

In order to empirically test our hypotheses we use details about the criminal records of the candidates available thanks to a 2003 Indian Supreme Court judgment that made it mandatory for every candidate contesting state and national elections to provide sworn affidavits. These include details not only about their personal, educational, and financial particulars, but also detailed information on any criminal charges they had faced, the status of their criminal cases and any charges against them.[3] We use criminal charges as a signal whether a MP is a criminal type. To alleviate potential bias in our estimates caused by measurement error due to individual false charges, we run all regressions both with a binary variable *Criminal* coded one for (a) those with a least one charge and (b) for only those with more than one charge. We study the 14^{th} (2004-2009) instead of the 15^{th} Lok Sabha (2009-2014) or a combination of both because a delimitation commission set up in 2002 changed the electoral boundaries of constituencies between the 14^{th} and 15^{th} Lok Sabha elections, making it impossible to match constituencies. Using the 14th Lok Sabha allows us to control for confounding factors such as past electoral performance or party strongholds which are crucial for MP effort.

We want to assess whether elected candidates with criminal records differ from their colleagues with regard to effort. Various measures have been used in the literature to gauge MP effort. Instead of picking just one factor, which might not capture differences between MPs comprehensively, we use three measures that each capture a different facet of MP behavior. First, we use attendance rates (respectively absenteeism) as for example in Besley and Larcinese (2011); Gagliarducci et al. (2010, 2011) and Mocan and Altindag (2013). Second, we make use of MPs' effort in parliament by considering information on the number of questions

[3] The court also asked the Election Commission of India to make it mandatory to publicize the information about electoral candidates provided through these affidavits. Voters can now use this information to make better informed electoral choices.

they asked and their participation in debates (cf. Mocan and Altindag 2013, Arnold et al. 2014). While both these measure capture effort, one might question their relevance for the electorate. Using economic outcomes like consumption directly as in Chemin (2012), on the other hand, is rather disentangled from MP behavior, which makes it more problematic to attribute differences to MP effort. Thus, we draw on Keefer and Khemani (2009) and use the cumulative utilization rate of the Member of Parliament Local Area Development Scheme (MPLADS henceforth). The fund is intended for the development of electoral constituencies and offers several advantages in making the effort that MPs make on behalf of their constituencies observable. While details are outlined below, some advantages are that the amounts available are identical across constituencies, implementation of projects requires substantial effort on behalf of the MP, projects are clearly identifiable with the MP's name and the considerable media coverage makes it likely that voters learn about the effort.

The observation level are the 543 constituencies, which each elect one MP in a first-past-the-post-system. We find that across specifications, *Criminal(a)* is related to around 5% higher absenteeism rates. Parliamentary activity, on the other hand, does not differ significantly between criminal and non-criminal MPs in our baseline specification. As suggested by our model, criminal MPs show both significantly lower attendance rates and less parliamentary activity in constituencies that are economically underdeveloped. The reason could lie in better monitoring in the more developed constituencies and/or the greater attractiveness of these constituencies for criminals in terms of rent-extraction possibilities. The coefficients become more negative when we use our *Criminal(b)* indicator instead. MPLADS fund utilization is lower for criminal MPs in general, but only significant for *Criminal(b)*.

The most obvious challenge for econometric identification is posed by omitted variable bias, in particular selection effects. Selection bias could arise if expected effort generally differs in those constituencies that have voted for a criminal MP. The direction of the bias is not ex ante trivial, as we can infer from our model. It would be negative, if, for example, less developed electorates are more likely to elect a criminal and it would be harder to recommend an MPLADS project in such a constituency. A positive bias would occur if less developed constituencies are more likely to elect a criminal and exhibit higher MPLADS utilization rates because it is easier to identify necessary projects. As the common *a priori* assumption is

a negative relation between *Criminal* and effort, we would be less concerned about upward bias, because our estimated negative coefficient would then be an upper bound of the true effect.

As part of our strategy to identify the causal effect of criminal type on the outcome variables, we first use the model as a theoretical foundation to derive an extensive set of relevant constituency- and MP-specific control variables. Second, fixed effects for major states ensure that the results are not driven by factors specific to certain Indian regions such as, for example, economic underdevelopment. Fixed effects for major parties ensure that the coefficient estimates are not driven by unobserved factors specific to a party or related to being part of the government or opposition. Third, we get identical results using alternative matching estimators and show that the control and treatment groups are strongly balanced. Results from treatment effect estimations that model the selection process explicitly yield slightly more negative estimates. For example, when controlling for selection, both *Criminal(a)* and *Criminal(b)* are significantly related to lower development fund utilization rates. This suggests that, if anything, omitted variables and/or selection effects seem to bias our coefficients for criminal background towards zero. For the negative relation between *Criminal(b)* and MPLADS utilization rates, we use selection-on-observables to demonstrate why this is, under relatively mild assumptions, an upper bound estimate of the negative effect. Moreover, we use methods developed in Altonji et al. (2005) to demonstrate that on average selection bias (on unobserved factors) would have to be between two and sixteen times greater than selection on observed factors to fully explain the negative relation between *Criminal* and attendance rates or MPLADS utilization. As we have plausibly identified the most relevant influencing factors in our model, it seems that criminal MPs are indeed detrimental to their constituency.

The rest of the paper is structured as follows: in the next section we summarize the relevant literature. We then present our theoretical framework yielding testable hypotheses on the impact of criminal background on parliamentarians' efforts in parliament and the development of their electoral constituencies. Section 4 describes our data, methods and estimation strategy. While section 5 presents the results and discussion, section 6 concludes and gives policy implications.

2 Literature

There is a growing literature concerned with the political system of India, and the many challenges that threaten the world's largest democracy, to which our paper connects. Besley and Burgess (2002) explore the relation between electoral accountability and the responsiveness of state governments to falls in production. Their model highlights the importance of information flowing about politicians' actions. Moreover, in particular the criminalization of politics in India has received much interest. A first strand of literature on this topic is focused on understanding why political parties field candidates with criminal backgrounds in elections in the first place and why voters elect these candidates. This issue has not been subject to intense scrutiny, although a handful of studies exist. Building on a theoretical model, Aidt et al. (2015) argue and find that political parties in India field candidates with criminal backgrounds when faced with intense electoral competition. One reason brought forward is that such candidates possess certain electoral advantages such as money and "muscle power", which they can use to influence the electoral outcome under conditions of low voter literacy levels and poor electorates.[4] In contrast, Vaishnav (2011) finds no evidence in favor of electoral competitiveness increasing the likelihood of criminal candidates being fielded by the political parties in India when examining 28 state elections.[5]

This does not mean that voters are unable to recognize this problem. Dutta and Gupta (2012) reveal that voters actually punish candidates with criminal charges that contest in elections.[6] This suggests that one issue might be the intensity of monitoring by voters: if monitoring costs are too high few might be aware of the candidates' characteristics. In addition, the effects tend to vanish when there are other candidates in the constituency with criminal charges. Under such conditions, the vote share gained by criminal candidates and candidates with enormous declared wealth also tends to increase. These findings are contradicted by Banerjee et al. (2009). Their evidence in a field experiment suggests that voters in rural India tend to vote on caste (ethnic) considerations even after being provided with information on the criminal background details of the contesting candidates. This suggests that positive preferences for certain characteristics that criminals possess can be enough to trump anti-corruption efforts and help criminal candidates get elected.

[4] See Hanusch and Keefer (2013) for a review of literature on when and why vote buying prevails in democratic societies.

[5] However, he does find the personal wealth of criminal candidates to be correlated with the criminal status of the candidates, suggesting that they could have accumulated wealth over their years of criminal activities. A similar correlation is found by Paul and Vivekananda (2004), who review the information provided by the candidates contesting in the 2004 national elections in India.

[6] Similar such findings in other countries are echoed by Brollo (2010) and Ferraz and Finan (2011).

The second strand of literature focuses on the consequences of electing candidates with criminal backgrounds. In their state-level analysis covering a period of over 20 years, Kapur and Vaishnav (2011) show the ominous nexus between the candidates contesting elections (especially the ones with criminal background) and the construction sector. Often, the candidates contesting in elections stash their illegal money and assets with builders in real estate in return for quid pro quo benefits. Prior to elections, the illegal money parked in the real estate sector is routed to fund election campaigns for these criminal politicians. Kapur and Vaishnav argue that as a result of this quid pro quo deal the economy grows less in these years, as measured by a reduction in the consumption of cement and other indispensable raw materials.

The studies that come closest to ours are Chemin (2012) and Prakash et al. (2014). Chemin (2012) examines the relationship between parliamentarians in India with criminal background and consumption levels in their respective constituencies. He finds that poverty levels tend to be higher and consumption lower in constituencies which are repesented by MPs with criminal backgound. The paper by Prakash et al. (2014) examines the economic consequences of Members of State Legislative Assembly (MLAs) with criminal background. Using satellite nightime light data across 20 major states in India, they find that electoral constituencies represented by MLAs with criminal background see a lower level of economic development measured by nighttime light data. The main drawback of these papers is the lack of explicit transmission mechanisms through which these effects are realized. In general, it is not obvious from the existing literature whether electing candidates with criminal backgrounds can explain differences in parliamentarians' efforts in parliament and the development of their constituencies. There is a vast empirical and theoretical literature on factors determining the performance of legislators. For instance, Svaleryd and Vlachos (2009) and Strömberg (2001, 2004) study the effect of political competition on economic outcomes. Fisman et al. (2014), Ferraz and Finan (2011), and Snyder and Strömberg (2010) examine political competition, media coverage and rent seeking behavior of incumbent politicians. More closely related, Aidt et al. (2015) modeled why parties choose to field a criminal in the first place and Dutta and Gupta (2012) tested empirically how voters respond to criminal charges. Our model thus extends the existing literature by examining whether and how the criminal background of an elected MP is related to differences in their effort in parliament and in developing their electoral constituencies.

3 The Model

We model the interplay between an incumbent parliamentarian and their electorate in a two-period model, similar to Besley and Burgess (2002). Consider an incumbent who was voted into office in a specific constituency at the beginning of period 1. At the end of period 1, the incumbent faces the election for the next legislative period. Voters base their decision in two dimensions: Personal characteristics and political effort exerted by the MP for their constituency.

The personal characteristics of a politician can be understood as capturing anything which can influence the voting decision of a citizen, including gender, age, wealth, party membership et cetera. Suppose there are n such personal characteristics and each characteristic can be expressed in a binary manner (i.e., male or female, old or young et cetera). Then, we may represent the personal characteristics of the incumbent as a n-dimensional vector x, where, for $k \in \{1, \ldots, n\}$, $x_k = 1$ if the incumbent exhibts characteristic 'k' and $x_k = 0$ if she does not. The facet we will focus on later is whether an incumbent is faced with criminal charges. In our analysis, we consider two types of incumbents who exhibit identical personal characteristics except that one has criminal charges against her and the other does not. We refer to the former as criminal (c), and to the latter as non-criminal (n), where we denote the corresponding personal characteristic vectors by x^c and x^n. Let s be the characteristic which represents 'criminal charges', then it holds that $x^c_s = 1$, $x^n_s = 0$ and $x^c_k = x^n_k$ for all $k \neq s$.

In period 1, the politician chooses her effort level $e \in [0, 1]$ which represents time allocated to political work. 'Effort' can be understood broadly as political activity that can be related to her position as the representative and advocate of her constituency. Marginal time costs are assumed to be 1 and linear for simplicity.[7] The reelection probability of the MP, $P(e, x)$ is influenced both by her political effort and her personal characteristics. Let $U > 0$[8] be the incumbent's utility from holding office, then an MP's optimization problem takes the following form:

$$\max_{e \in [0,1]} \; P(e, x) \cdot U - e \tag{1}$$

[7] Our results hold for any convex cost function.

[8] The utility from holding office is assumed to be strictly positive. Otherwise, a rational individual would not run for public office.

Voters can learn the effort level of the MP, but learning is costly. We denote the fraction of voters who choose to be informed ι. Being informed requires sufficient access to sources like electronic or print media and the ability and willingness to comprehend the information. Therefore, it seems plausible that a share of voters $(1-\iota)$, which we can illustratively think of as illiterate, poor or politically less interested, cannot afford or chooses not to learn. However, voters who belong to the latter group are not necessarily completely uninformed. They do not learn the effort level of the MP, but may still know some obvious and easily accessible personal characteristics x_k of the incumbent and her challengers such as gender, party and caste membership. However, the intensity of monitoring of MP activity is clearly higher with a higher share of informed voters.

The incumbent has the possibility to convince a share of the informed voters to vote for her by exerting political effort in period 1. While we assume a specific functional form of this relationship in this section for illustrative purposes, Appendix A shows that the results in Propositions 1 and 2 still hold with more general functions. Let $I(e,x) = \frac{e}{e+m(x)}$ be the fraction of informed citizens who vote for the incumbent where m is a function from the the space consisting of all possible characteristic combinations into positive reals.[9] Function m can be regarded as a measure for the electoral competitiveness in the MP's constituency. It determines a proportion of the informed population, $\frac{m}{m+1}$, who would not vote for the MP regardless of her effort. The underlying reasons are voters' preferences over personal characteristics.[10] These preferences are constituency-specific: whether the membership in a party A increases or decreases m depends on voters' preferences for A in the respective constituency.[11]

There is convincing evidence that a significant part of the Indian population is generally opposed to political criminality (Banerjee et al., 2014). While other reasons contribute to the elections of criminals, Dutta and Gupta (2012) find that, all else equal, voters penalize candidates with criminal charges. In

[9] $I(e,x)$ is similar to the function of Tullock (1980). However, in our model, it represents a fraction of the informed population and not a winning probability.

[10] We have not specified the preferences of voters for simplicity. One may think of, for instance, Euclidean preferences over personal characteristic: Let α_v be voter v's ideal point in the characteristic space, then voter v evaluates a characteristic profile x by $-\parallel \alpha_v - x \parallel$. The existence (resp. non-existence) of a certain characteristic k increases m if k is contained in the ideal points of the majority (resp. minority) of voters.

[11] Because it is not directly relevant for our purpose, we do not model challengers and candidate selection explicitly. It is of course likely that challengers of the incumbent engage in election campaigns. It is plausible, however, to distinguish these election campaigns, which only transport a promise about effort, from the actual political effort which only the incumbent can exert due to his role as an MP. Rather, the strength of candidate competition would enter as a factor in m. The lower m, the higher the fraction of informed voters who can be convinced by a certain effort level of the MP. The empirical application will control for such factors.

line with the empirical evidence, we impose two assumptions. Firstly, by exerting a specific effort level, a non-criminal MP can convince a larger proportion of the population than a criminal, $m(x^c) > m(x^n)$. Secondly, effort of non-criminals has a higher marginal impact, since voters take a more skeptical stance about the political effort of criminals. This is captured by assuming $m(x^c) \cdot m(x^n) \geq 1$ which implies that $I_e(e, x^n) > I_e(e, x^c) > 0$ for $e \in (0, 1)$.[12] Note that $I_{ee}(e, x^n) < I_{ee}(e, x^c) < 0$ which means that the difference $I_e(e, x^n) - I_e(e, x^c)$ decreases in e. The intuition here is the following. Voters skepticism towards criminals diminishes with higher effort levels, as high political effort is perceived as s stronger and more reliable signal regarding the future effort of the MP.

The fraction $(1 - \iota)$ of uninformed voters, on the other hand, cannot be convinced by political effort, since they do not learn about effort. Instead, uninformed citizens vote randomly to some degree. Following Besley and Burgess (2002), we assume that the fraction N of uninformed citizens who end up voting for the MP is uniformly distributed on an interval $[a, 2b(x) - a]$ where $1 > b(x) > a \geq 2b(x) - 1$. As argued above, while informed voters choose not to learn about the MP's effort, they still possess information about the candidates like, for example, her name and party membership which are visible to everyone on the ballot sheet. The function $b(x)$ represents the expected level of support for the MP based on this information, a higher $b(x)$ relates to a higher expected vote share. The a represents noise in voting: The lower a the higher the variance, the higher a the lower the variance. One important aspect, which we have not explicitly incorporated in the model so far, is that criminals can use campaign practices which are not available to non-criminals such as the intimidation of voters or the buying of vote (e.g., Vaishnav, 2012). In the model, this would be best captured by assuming that criminal incumbents can push up the expected level of support of the uninformed citizens. Consequently, we will assume that $b(x^c) \geq b(x^n)$.

[12] We use the following standard notation for partial derivatives: $\frac{\partial f}{\partial x} := f_x$ and $\frac{\partial^2 f}{\partial x \partial y} := f_{xy}$.

In a first-past-the-post system, the incumbent wins the election in her constituency if[13]

$$\iota I(e,x) + (1-\iota)N > \frac{1}{2}.$$

By using this condition, one obtains the winning probability of the MP:

$$P(e,x) = \begin{cases} 1, & \text{if } \frac{-1/2+\iota I(e,x)+(1-\iota)a}{(1-\iota)2(b(x)-a)} > 0 \\ 1 + \frac{-1/2+\iota I(e,x)+(1-\iota)a}{(1-\iota)2(b(x)-a)}, & \text{otherwise} \\ 0, & \text{if } \frac{-1/2+\iota I(e,x)+(1-\iota)a}{(1-\iota)2(b(x)-a)} < -1 \end{cases}$$

The incumbent wins the election for sure if $(1-\iota)a > 1/2$ and loses for sure if $\iota\frac{1}{1+m(x)} + (1-\iota)(2b(x)-a) < 1/2$. In both cases, the optimal effort of the MP is zero. Furthermore, if there exists an effort level $\hat{e} \in (0,1)$ such that $P(\hat{e},x) = 1$, it is obvious that the optimal effort level of the incumbent will never exceed $\hat{e}$. We focus on the other cases. The first-order condition of equation (1) is $P'(e,x) \cdot U = 1$ (i.e., the marginal returns to effort equal its marginal costs).[14] This yields

$$z_j = \sqrt{\frac{\iota}{1-\iota} \cdot \frac{U}{2(b(x^j)-a)} \cdot m(x^j)} - m(x^j) \tag{2}$$

where $j \in \{c,n\}$.

The optimal effort level of type j is $e_j^* = z_j$ if $z_j \in [0,1]$, which we will refer to as the interior solution in the sequel. If $z_j < 0$, then $e_j^* = 0$, and if $z_j > 1$, then $e_j^* = 1$. Assuming an interior solution, we at first consider the impact of the constituency-specific parameters on the optimal effort level regardless of incumbent type. The results are summarized in our first proposition below. Afterwards, we compare the optimal effort of a criminal and a non-criminal incumbent.

Proposition 1. The optimal effort level of the incumbent is higher if

(i) voters are better informed (high ι)

(ii) the MP's utility from holding office is higher (high U)

[13] We simplify by focusing on two candidates for illustrative purposes. Extending the model with more candidates would not affect our main conclusions and introduce unnecessary complexity.

[14] Note that there are two effort levels which solve the first-order condition of equation (1). However, the other fails the second-order condition.

(iii) the expected level of support is lower (low b)

Proof. The proof is straightforward by using equation (2) □

Results (i) and (ii) make intuitive sense and do not require much interpretation. Result (iii) together with the result for the case of a certain election victory or defeat can be interpreted as effects of electoral competitiveness. Thus, our model can also help to explain the finding by Keefer and Khemani (2009) that effort levels are generally lower in party stronghold constituencies with little competition. The reason for (iii) is that a lower $b(x)$ decreases the length of the interval for the uniform distribution, i.e. the share of voters who vote randomly. A shorter interval results in less variance in the expected winning probability, hence the marginal effect of effort increases.

As described above, function m differs for a criminal and a non-criminal incumbent. The difference between a non-criminal and criminal incumbent is specified in Proposition 2.

Proposition 2. The difference between optimal effort levels $\Delta_{e^*} = e_n^* - e_c^*$ is

(i) strictly positive

(ii) decreasing in ι

(iii) decreasing in U

Proof. (i) By result (iii) of Proposition 1, if (i) is true for $b(x^c) = b(x^n)$, then it is true for $b(x^c) > b(x^n)$. Thus, suppose $b = b(x^c) = b(x^n)$. Then, the first-order condition of equation (1), for $j \in \{c, n\}$, is $I_e(e_j^*, x^j) = \frac{(1-\iota)2(b-a)}{\iota U}$. Hence, it holds that $I_e(e_n^*, x^n) = I_e(e_c^*, x^c)$. It follows that $e_n^* > e_c^*$, since by assumption it holds that $I_e(e, x^n) > I_e(e, x^c)$ and $I_{ee}(e, x^n), I_{ee}(e, x^c) < 0$ for all $e \in (0, 1)$. Thus, Δ_{e^*} is strictly positive. (ii) Again, suppose $b = b(x^c) = b(x^n)$, then by using equation (2) and deriving the distance with respect to the information level, we obtain $\frac{\partial \Delta_{e^*}}{\partial \iota} = \frac{1}{2(1-\iota)^2} \cdot \sqrt{\frac{(1-\iota)\cdot 2(b-a)}{\iota \cdot U}} \cdot \left(\frac{\sqrt{m(x^c)} - \sqrt{m(x^n)}}{\sqrt{m(x^n) \cdot m(x^c)}} \right)$ where the first and the second term are strictly positive and the third term strictly negative, since we assumed $m(x^c) > m(x^n)$. Thus, $\frac{\partial \Delta_{e^*}}{\partial \iota} < 0$, which corresponds to statement (ii). Statement (iii) can be shown in analogy. □

When we test these hypotheses empirically, we propose that a high information level as well as utility in form of rent-extraction potential are related to the development level of the constituency.[15] Results (i)-(iii) of Proposition 2 can be interpreted as follows. In constituencies with a low development level, criminal incumbents put considerably less effort in political work than non-criminals. It is not implausible that criminal incumbents derive higher utility from holding office in electoral districts that are economically more developed, because these offer greater potential for rent extraction (cf. Fisman et al. (2014), who document the growth in incumbents assets while holding office).[16] Thus, if the development level increases, the effort of a criminal converges to the effort level of a non-criminal MP.

To summarize, the model shows how MPs' criminal backgrounds can relate to their chosen effort level, by taking account of re-election concerns and incorporating informed and non-informed electorates. We derived that criminal types exert less effort. However, the model suggests that a higher share of politically informed voters increases the incentives to engage in effort for all types of incumbents. The model rests on relatively mild and general assumptions, and can be aligned with previous empirical findings and related models. For our empirical application, the model provides useful guidelines for the selection of variables and testable hypotheses.

4 Data and empirical strategy

We use various data sources to construct a constituency-level data set for the 14^{th} Lok Sabha legislative period. We focus on the 14^{th} (2004-2009) instead of the 15^{th} Lok Sabha (2009-2014) or a combination of both because a delimitation commission changed the electoral boundaries of constituencies between the 14^{th} and 15^{th} Lok Sabha elections in 2002, which makes it impossible to match constituencies. Using the 14th Lok Sabha allows us to control for confounding factors such as past electoral performance. This section describes our proxies for the effort level chosen by the incumbent MP, our measure whether an MP is of the criminal type, as well as the proxies for electoral competitiveness, monitoring intensity and candidate

[15] Rent-seeking of politicians in power is by no means limited to developing countries. For instance, see Kauder and Potrafke (2015) for a documented case of rent extraction on part of elected members of parliament in the German state of Bavaria.

[16] We have not modelled this explicitly, since the implications are obvious. The reasoning is as follows. Suppose that U depends on personal characteristics x and on the normalized GDP of the constituency $g \in [0,1]$ such that $U_g(x_n, g) = 0$ and $U_g(x_c, g) > 0$. Then, by the first-order condition of equation (1), the higher g, the higher the effort of a criminal MP.

characteristics (Table 1 provides descriptive statistics). We use two different measures to gauge the parliamentary performance of MPs, and one indicator to assess constituency development proposed by Keefer and Khemani (2009). All three have the advantage that they can be directly attributed to actual MP effort.

4.1 Dependent variables:

i) Attendance rates and parliamentary activity

The most obvious measure of MP effort is *attendance rates* in parliament. This measure has several advantages. First, it is easily measurable and clearly interpretable. Second, it has been widely used in the literature, for example in Gagliarducci et al. (2010, 2011) and Besley and Larcinese (2011). Mocan and Altindag (2013) and Fisman et al. (2014) use it as their main measure of effort in studies on MPs in the European parliament. To avoid confusion, note that some papers use the absenteeism rate instead, which is of course simply the inverse of our measure. Our variable *attendance rate* is scaled between zero and one. The lowest rate is 6% for former prime minister Atal Bihari Vajpayee from Uttar Pradesh, who has no criminal background, but was already 76 years old at the date of election. The highest rates are 96% for two MPs from Bihar and Manipur, both without any criminal charges against them. The simple correlation between *Criminal(a)* and *attendance rate* is -0.14.

Though *attendance rates* as a measure has many benefits, it does not necessarily correlate with MPs' work attitudes and intensity once they are actually present. Therefore, we complement our analysis of MP effort by a second measure of MP *parliamentary activity* within the parliamentary sessions in the 14^{th} Lok Sabha period. The literature has for example suggested using speeches, oral contributions and private initiatives (cf. Arnold et al., 2014) or the number of questions asked (Mocan and Altindag, 2013). We combine two categories, the number of questions asked and the number of debates in which MPs have participated,

into one indicator named *parliamentary activity*.[17] It is more likely that voters receive a signal, whether it be via personal investigation or via the media, about the average effort invested into activities by their MP. Hence, an overall indicator is better suited to capture the total effort exerted by an MP inside the parliament and proxy the effort level observed by the voters. We normalize each indicator by dividing it by its standard deviation to achieve comparability, and then take the simple average. This aggregate indicator ranges between 0 for nine MPs who have neither asked any questions nor participated in any debate, to 5.03 for C.K. Chandrappan from Trichur constituency in the state of Tamil Nadu, who asked 415 questions and participated in 113 debates. There is no obvious correlation between *parliamentary activity* and criminal type, the simple correlation with *Criminal(a)* is a mere 0.003. The data for both *attendance rate*s and parliamentary activity exerted by MPs is taken from the Association for Democratic Reforms (ADR), an independent body that researches Indian elections which was established in 1999.[18]

ii) MPLADS utilization rate

Our third dependent variable is intended to capture MPs' efforts in developing their respective electoral constituencies.[19] We follow Keefer and Khemani (2009) who use utilization of Member of Parliament Local Area Development Scheme (MPLADS) funds meant for development of MPs' constituencies. Introduced in 1993, each MP can receive about 10 million Indian rupees (about 160,000 $US) annually to spend on developmental activities or on local public works recommended by the MP of that constituency. In 1998, it was increased to 20 million Indian rupees. Any unspent money under the MPLADS fund accumulates and is carried forward to the next fiscal year until an MP leaves office. The new MP representing that constituency will inherit the total unspent amount under MPLADS.

The utilization of funds from the MPLADS is a good proxy for the effort exerted by an MP to develop

[17] We do not use the proposition of private member bills. In the Indian parliamentary system any MP not acting on behalf of the government or political party can introduce a bill in the parliament with the permission of the speaker of the house. The speaker, in consultation with the leader of the house (i.e. the Prime Minister), allots two and half hours on every Friday in each of the parliamentary sessions to discuss the private bills proposed by the MPs. So far, 14 private members bills have been passed in Indian parliament. All of these bills were passed before 1970. Since 1970, not a single private members bill was passed. During the 14th Lok Sabha period a total of 300 private members bills were moved by various MPs, of which a mere 4% were actually discussed (see Kumar 2010).

[18] ADR is collecting relevant details about candidates contesting both national and state-level elections in India. See: http://adrindia.org/research-and-reports/election-watch

[19] Note that electoral constituencies in India do not overlap with districts' boundaries in the states. There is no easily applicable procedure to aggregate districts up to constituencies.

her constituency for several reasons. First, it is noteworthy that the amount (20 million Indian rupees) allotted to each MP every year is independent of an MP's effort and is identical across all the MPs. Utilizing these funds to develop the constituency is purely the prerogative of respective MPs representing their constituencies because the public works which are of utmost importance for the development of that constituency must be identified and initiated by the MP alone. This effectively means that MPs need to exert considerable effort to undertake various developmental works: they must work in tandem with various government bureaucrats at the national and state level to first identify viable projects and then obtain permissions and sanctions for the work and monitor the work once the project is undertaken. Second, the MPLADS permits MPs to take credit for the public works projects undertaken under this scheme. This provides incentives for the MPs to exert effort to develop their constituency. Third, Keefer and Khemani (2009) demonstrate that in the recent years voter awareness of the MPLADS has increased. Thus we can credibly assume that informed voters are aware of their MP's effort. MPLADS utilization is hence a measure of direct relevance for citizens, but in contrast to consumption (Chemin, 2012) and nighttime light intensity (Prakash et al., 2014) can directly be traced back to MP effort.[20]

We use the cumulative utilization rate which is the actual spending incurred by an MP in her constituency as a percentage of the total amount released under the MPLADS each year during the 14th Lok Sabha period. Unfortunately, the data on actual spending under MPLADS are not publicly available for the year 2005. Thus, our cumulative utilization rate includes the data from 2006 to 2008. Note that the actual spending incurred by an MP includes any unspent amount which is inherited from her predecessor. Therefore a higher utilization rate must denote greater effort being exerted by the MP to bring development to their respective constituencies. We obtain the data on the MPLADS funds from the annual reports on the MPLADS published by the Indian Government's Ministry of Statistics and Program Implementation.[21]

The distribution of the dependent variables deviates from a normal distribution to some degree (see Appendix B, Figure 1). We will thus replicate our baseline models with the propensity score matching estimator, which requires fewer distributional assumptions. Potential severe outliers might in particular be very high values in *parliamentary activity* and *MPLADS utilization*. We hence also re-estimate our models without

[20] For more details on MPLADS, see: http://mplads.nic.in/welcome.html, accessed between March and November 2013).
[21] See: http://mplads.nic.in/Annualreportmenu.htm, accessed between March and November 2013)

these potential outliers in the robustness section.

4.2 Variable of interest

Our key independent variable is the criminal background of MPs. With the Supreme Court's 2003 order, all candidates contesting state or national elections in India are required to submit a sworn affidavit detailing their criminal background to the Election Commission of India. These are available to voters on the Election Commission's website.[22] They provide information about the number and types of criminal accusations against a candidate, and their number of convictions. We make use of this information to create a binary variable *Criminal(a)* which takes the value of 1 if an MP has any accusation against them and 0 otherwise.

Our aim is to measure the criminal type of an incumbent. Criminal charges provided in the affidavits constitute a good, but imperfect proxy. First, some of the cases registered against the candidates could be politically motivated.[23] However, Vaishnav (2011) argues that information disclosure about criminal charges is obligatory only if the judge deems the charge worthy of a criminal proceeding after a thorough investigation by the local police. Second, candidates may under-report their criminal charges. Still, the potential political costs of under-reporting are high as it is prosecuted and opposition parties may latch on the issue, leading to disqualification of the member from the parliament. Still, while we want to identify the MP's true type CR^*, we observe only the affidavits, which transport a noisy signal $CR = CR^* + u$. Measurement error in CR would attenuate its coefficient, i.e., bias it towards zero.

To avoid such problems we also code a variable *Criminal(b)* that only takes the value of 1 if an MP has

[22] See: http://eci.nic.in/eci_main1/LinktoAffidavits.aspx and http://eci.nic.in/archive/GE2004/States/index_fs.htm, accessed between March and November 2013. In some cases it was necessary to manually adjust the spelling of names in the different data sources. This was done by comparing the names with the information available at http://ibnlive.in.com/politics/cand2004.php and adjusting the names accordingly. See Appendix B, Figure 2 for an example of such an affidavit.

[23] While anecdotal, speaking to people which are familiar with the issue or involved in politics gives the clear indication that most charges are in deed justified. The main reasons why so many charges are still pending is the fact that the Indian judiciary system is notoriously overburdened and that it takes years until a specific case is finally dealt with in courts. If candidates or MPs are convicted of a crime, they are no longer allowed to run for office, and thus not in our sample. In some sense, only the slow processing time of the Indian courts allows us to observe these supposedly criminal actors in their parliamentary role. We are not aware of another country with this combination of both a large number of criminals in politics and the general requirement to publish pending criminal charges which take a long time to be dissolved. Thus, while we remain cautious with regard to external validity, we hope that our analysis also reveals relationships that could be relevant for other countries but cannot be observed there.

more than one charge against him. This alleviates concerns about mistaking innocent MPs for criminals insofar as it is less likely that all charges are wrong. Also, for some of the accused MPs, their illegal activities might have been a one-time, unrepeated mistake. *Criminal(b)* is more likely to capture "real" criminal types. The main advantage of this coding approach is its simplicity and its objectivity compared to subjectively rating the relative severity of crimes (See Appendix B, Table 1 for frequencies and details).[24]

MPs with criminal accusations against them are not a phenomenon bounded to few states or certain parties. Table 2 shows that all parliamentary parties are comprised of some criminal members. The share is highest for Rashtriya Janata Dal, a party most prominent in the state of Bihar, with 10 out of 21, or 47.6%, of members being accused of criminal activity. Of the other major parties, the shares range from 15.6% for Indian National Congress to 29% for the Samajwadi Party. The geographic distribution is equally dispersed, as can be seen in Figure 1. Most states have at least one and usually more MPs facing criminal accusations. The highest shares in the major states are to be found in Kerala with 36.8%, Bihar with 38.7% and Maharashtra with 46.2%. Assam is the only large state without any such MP, and in general the far east of India seems to be mostly free of MPs accused of criminal activity (See Appendix B, Table 2 for details).

4.3 Control variables

Our control variables fall into the three categories electoral competitiveness, monitoring intensity and candidate characteristics. A candidate's personal characteristics can affect their individual re-election probability, which relates to the function $m(x)$ in our model.[25] The affidavits also include details about

[24] Some crimes could be thought of as more directly related to shirking and negative effects for a criminal's home constituency. Crimes related to corruption like accepting bribes might be particularly problematic in a political context. The problem with this is that it is hard to distinguish crimes unequivocally into those relevant for shirking and/or parliamentary performance and those which are not. Murder for example could be related to pure greed and robbery, but also be committed or commissioned to achieve political goals. Within our sample there are not enough cases of crimes that are clearly related to politics like corruption; most crimes are in fact capital crimes. A second difference to *Criminal(a)* could be that *Criminal(b)* captures differences in the abilities of 'criminal' MPs, who must not necessarily form a homogenous group, to intimidate and bribe voters. If the latter group can acquire more voters that way, they would also engage in relatively less effort.

[25] Please note what it theoretically means to control for candidate characteristics. As outlined in the model, criminal MP's effort can differ due to the reaction of informed voters to criminal background or due to vote-buying. However, there might also be unobserved characteristics that differ between criminals and non-criminals. For example, criminals could differ in their ability, but also simply score higher on a laziness scale (reflected in higher costs of effort in the model). We show results with and without observable proxies for ability and other characteristics. If a potential relationship remains significant conditional on the candidate characteristics we use, the remaining channels that explain this difference could be either the two outlined in the model, or some unobserved difference in character that relates to criminal status. We will further discuss the potential influence of unobserved factors later.

candidates' total assets and liabilities, educational qualifications, age, gender, and experience in parliament. For age, we use MP age at the time of election in 2004. We dummy code the gender variable giving the value 1 if an MP is male and 0 if female. For education we create an ordinal three category system which assigns a value of 0 if education is not given or indicated as "other" or "literate", 1 if the educational achievement is between the 10^{th} to 12^{th} grade passed, and 2 for all graduate, post graduate or other graduate attainments. For MPs' experience we use a simple count of number of times the MP has been elected before 2004. It is also a proxy for incumbency advantages or disadvantages, but provides a more nuanced measure of experience which could foster effectiveness in parliamentary work. We calculate net assets as the difference between assets and liabilities, add the minumum net assets plus the value one to all observations to avoid the creation of missing values, and take the logarithm.

In regards to constituency features related to competitiveness and monitoring intensity (the share of informed voters ι), we first control for voter turnout as a proxy for the extent to which voters within a constituency are interested in and informed about politics. A similar argument holds for literacy rates: Aidt et al. (2015) suggest that illiterate voters might be less put off by criminality and easier prey for vote buying and intimidation tactics. Likewise, we also control for the closeness of electoral races in constituencies. We use the margin of victory in terms of difference in the vote share received by an MP against the immediate runner-up in 2004 to control for electoral competitiveness. Keefer and Khemani (2009) argue that it is also a measure of voter attachment. To address this further, we include a variable capturing whether or not a constitutency is a party stronghold. This dummy takes the value 1 if a candidate belongs to a political party that has won elections in that constituency for the last three successive elections in 1996, 1998 and 1999. We also use a dummy variable for those constituencies which are reserved for candidates from Scheduled Castes (SCs) and Scheduled Tribes (STs). [26]

Finally, we use the sum of night lights as a proxy for the economic development of the constituencies. As constituency boundaries do not coincide with the administrative district boundaries there are no official GDP estimates at the constituency level. Henderson et al. (2011), among others, demonstrated how to calculate such a measure and show that it correlates with official GDP growth numbers. Chaturvedi et al.

[26] In these constituencies, only members of the respective castes and tribes can be elected into office. All data were collected and coded from publicly available sources, mostly the Election Commission of India. Data on partywise competition since 1977 come from http://eci.nic.in/eci_main1/election_analysis_ge.aspx .

(2011) used nighttime light for a study on income distribution within India. They emphasize that light data have an additional advantage as an objective measure of economic development in countries where official data are either not always available or cannot always be fully trusted. We follow the usual approach and use average visible, stable light on cloud free nights, collected by the F16 satellite for the year 2004.[27] We use the tif-image-file from the National Geophysical Data Center and merged it in ArcGIS with constituency boundaries that were shared by Aidt et al. (2015). We then calculated the sum of lights using zonal statistics within the constituencies to proxy for economic development. In more developed constituencies voters have better access to media, are more likely to be interested in politics and hence more likely to be informed about their MP's performance.

The resulting maximum sample size for our estimations varies between 395 and 439. The first reason is changes of the MP during a term period, the possible reasons for which are manifold: From promotion into minister or other superior positions at the state level, planned resignations within the period, expulsions, to the death of the MP. We carefully checked each of these cases with information from the election commission of India and excluded all changes.[28] Data on *attendance rate* and *parliamentary activity* are only available in aggregate form over the legislative period, hence comparing MPs with two years in the Lok Sabha to those with four years is misleading. Appendix B, Table 3 shows that a dummy variable coded one in cases where there was a change is not significantly related to either the *Criminal(a)* dummy or the *MPLADS utilization rate*. Hence, dropping out of the sample is not systematic in a problematic way and hence does not pose a concern for our estimations. The second reason is that for six constituencies the affidavits could not be accessed either due to poor quality of the scan or malfunctioning links that could not be repaired. Third, sample size is constrained by our first two dependent variables which are available for 395 out of the 435 constituencies left in the sample.

[27] For more on this measure, see: http://ngdc.noaa.gov/eog/gcv4_readme.txt. The original description states that "the cleaned up (file) contains the lights from cities, towns, and other sites with persistent lighting, including gas flares. Ephemeral events, such as fires have been discarded and background noise was identified and replaced with values of zero. Data values range from 1-63. Areas with zero cloud-free observations are represented by the value 255". Appendix B, Figure 3 graphically depicts the geographic variation of economic development in India.

[28] http://eci.nic.in/archive/GE2004/States/index_fs.htm, accesssed between September and December 2014.

4.4 Empirical Strategy

We distinguish between the analysis of the dependent variables related to parliamentary work, *attendance rates* and *parliamentary activity*, and the one relating to *MPLADS utilization*. Both measure slightly different dimensions of MP effort. The former two relate very directly to input and effort, whereas the latter also relates to output and MP effectiveness in promoting the development of their constituencies. We refer to the dependent variables as Ef_i, the effort of an MP in constituency i. What we aim to measure is the Treatment effect on the treated (TOT), where Treatment consists of the MP being of the criminal type ($Criminal(Cr) = \{0, 1\}$). The observed differences in the data might, obviously, capture the TOT, but be affected by selection bias.[29]

$$E[Ef_i|Cr_i = 1] - E[Ef_i|Cr_i = 0] \quad =$$

$$\overbrace{E[Ef_{1;i} - Ef_{0;i}|Cr_i = 1]}^{ATET} + \overbrace{E[Ef_{0;i}|Cr_i = 1] - E[Ef_{0;i}|Cr = 0]}^{SelectionBias}$$

The coefficients related to *attendance rates* and *parliamentary activity* as proxies for Ef_i would be upward biased, for example, when constituencies that are more likely to elect a criminal are also those that engage in less monitoring of MP activities. Upward biases ($\mathrm{E}[\mathrm{Ef_{0;i,t}}|\mathrm{Cr_i} = 1] - E[Ef_{0;i}|Cr = 0] > 0$) would also occur when certain constituencies are both more likely to elect a criminal MP and have higher MPLADS utilization rates. For example, poor constituencies with a lower information level could be more likely to elect a criminal MP, and at the same time it is easier to think of and recommend a project in a less developed environment. In this case, our estimates would be biased against finding a negative effect of *Criminal*. Downward bias ($\mathrm{E}[\mathrm{Ef_{0;i,t}}|\mathrm{Cr_i} = 1] - E[Ef_{0;i}|Cr = 0] < 0$) is possible if it would, for instance,

[29] Biases could potentially exist if having a criminal type MP and political effort or outcomes are simultaneously determined equilibrium outcomes. We are not explicitly modeling this, but Aidt et al. (2015) and Dutta and Gupta (2012) explain the underlying dynamics in more detail. Our aim is to assess how likely it is that these potentially disturbing factors affect our estimations and in which direction. We will also show how large this selection-on-unobservables would have to be to account for our estimated coefficients.

be harder to find and develop projects in constituencies that are more likely to have a criminal MP.[30]

Our first attempt to avoid selection bias is, as usual, by carefully selecting an extensive set of control variables and relying on the conditional independence assumption. In doing so, our theoretical model provides guidelines as to the areas from which to select relevant control variables. We estimate

$$Ef_i = b_0 + Cr_i b_1 + X_i^{'} b_2 + S_s + P_p + \varepsilon_i$$

where Ef_i indicates effort in one of the three dimensions in constituency i, *Criminal*(Cr_i) is our dummy for whether the MP has a criminal background, X_i is the matrix of control variables in the three categories electoral competitiveness, monitoring intensity and candidate characteristics as specified above, and S_s and P_p are dummies for states and parties respectively. We follow Keefer and Khemani (2009) and use dummies for all major states. As outlined above, criminal MPs are found all across India, but some of the larger states obviously exhibit a higher percentage than others. With the fixed effects we make sure our results are not caused by unobservable, time-invariant factors that are specific to, for example, Maharashtra which has the highest share of MPs with criminal charges.[31] With regard to parties, we choose to employ party dummies as additional controls for all parties that are comprised of twenty or more MPs. The distribution of MPs by party and the respective share of *Criminal* is provided in Table 2.

While we demonstrate in our model that it is not obvious that MPs with criminal charges actually execute less effort than others, the press coverage, as well as public opinion and existing research, suggest a negative coefficient for *Criminal*. If this a priori assumption is true, we would be less concerned about upward bias in the coefficient of *Criminal* ($\mathrm{E}[\mathrm{Ef}_{0;\mathrm{i}}|\mathrm{Cr}_{\mathrm{i}} = 1] - E[Ef_{0;i}|Cr = 0] > 0$). Our estimates would then pose an upper bound of the (more negative) causal effect. We will show various pieces of econometric evidence in the analysis that suggest upward bias is more likely than downward bias.

[30] Note that the MPs effort in the parliament should not be affected by the time spent on preparing for the court cases. The Indian judiciary system allows the accused to appoint a lawyer (either public prosecutor or a private lawyer) to defend her case in the court of law. This effectly means the accused need onlybe available in the court of law on certain important matters such as questioning the accused or the day of pronouncing the verdict the accused must be present in the court of law.

[31] Smaller states like the union territories Andoman and Nicobar have only one or very few MPs, hence there is also no within-variation in the variable *Criminal*. A full list of the states and the respective share of criminal MPs is provided in Appendix B, Table 2.

We use cluster-robust standard errors that allow arbitrary within-cluster correlation. With regard to parliamentary work, correlations between individuals' efforts are arguably most likely within parties. Parties are the natural unit of comparison within a parliament; MPs are, for example seated along with their own party members. Thus, we choose the party level as the clustering unit for the first two variables. For the MPLADS fund, on the other hand, outcomes within states are more likely to be correlated and we cluster on the state level. The implementation probability and effectiveness depends on the individual state, which processes and executes the projects. Some states might, for example, be quicker in implementing the proposals than others; and differences in ex ante success probability can affect the likelihood of applying for a project. We now turn to our results.[32]

5 Results

5.1 Baseline

Table 4 in Appendix B depicts the baseline results for the regressions with *attendance rates, parliamentary activity* and *MPLADS utilization* as dependent variables. All regressions contain state dummies and dummies for the major national parties. Dummy coefficients are not displayed to improve clarity and readability. *Attendance rates* ranges from 0 to 1, *parliamentary activity* from 0 to 4.38 and *MPLADS utilizatio*n from 60.5 to 260.

Let's consider *attendance rates* first. The omitted reference category for the major party dummies are other or non-national party MPs. Positive relations with *attendance rates* compared to this reference category can be seen for the "Indian National Congress" and the "Samajwadi Party", significant at the 1% and 5%-level, respectively. For *parliamentary activity* the positive effect of "Indian National Congress" disappears and we observe a negative relationship with being a member of the "Communist Party of India".

[32] The number of clusters, 42 parties for parliamentary work and 33 states for the MPLADS fund, should be sufficiently high not to suffer from 'few cluster' inference problems. Our main results are virtually unchanged when clustering on either state or party. Recently, MacKinnon and Webb (2015) also suggested that inference might be affected by wildly different cluster sizes. We programmed a cluster wild bootstrap procedure based on the suggestions in their appendix and Cameron et al. (2011). To generate the bootstrap dependent variables we used the "Rademacher" 2-point distribution as well as the "Webb" 6-point distribution. The results with 10,000 repetitions mostly confirm the findings with more standard procedures. *Attendance rates* remains significant with *Criminal (a)* (Rademacher p-value=0.066/ Webb p-value=0.063) and *Criminal(b)* (Rademacher p-value=0.009/ Webb p-value=0.010), and *parliamentary activity* remains insignificant. The only difference is for MPLADS utilization rates, where *Criminal(b)* becomes marginally insignificant (Rademacher p-value=0.139/ Webb p-value=0.125).

It is positively related with the "Rashtriya Janata Dal" and the "Samajwadi Party". The only significant party for *MPLADS utilization* is a negative relation to "Indian National Congress".

For *attendance rates*, MPs from constituencies that are reserved for religious or caste minorities show significantly higher *attendance rates* of about 6% compared to their colleagues, significant at the 1%-level. Winning margin, proxying for lack of competitiveness, is as predicted negatively related to all measures of effort, and significantly so for *attendance rates*.[33] A ten percentage point increase in the winning margin translates into roughly two percentage points lower *attendance rates*. Party stronghold is not significantly related to effort in any of the specifications, conditional on controlling for winning margins. Voter turnout in 2004 is negatively related to effort, significant for *attendance rates*. The negative relationship is somehow surprising, but we should not forget that this is conditional on other proxies that might already capture the positive component of more monitoring already. Economic development, measured by luminosity, is significantly positively related to *parliamentary activity*, and literacy rate is significantly and positively related to *attendance rates*.[34]A 10% higher voter literacy rate translates into two percentage points higher *MP attendance rates*.

Conditional on all other factors, age and education are significantly and positively related to *attendance rates*. A five year age difference translates into about 1% higher *attendance rates* for the older MPs. Higher education also relates to less absenteeism: possessing a tertiary compared to only a secondary degree relates to about 2.5 percentage points higher *attendance rates*. Experience in parliament and Gender are not related to *attendance rates*; however, there is a negative relationship between Net Assets and attendance, which is significant at the 5%-level. In addition, being male seems to be positively related to parliamentary activity. We regard this intensive discussion of the conditional factors as justified due to the role they play in our model; however the rest of our tables will omit these coefficients to enhance clarity for the reader.

[33] The number of voters as a proxy for constituency size, which Aidt et al. (2015) related to the likelihood that a criminal MP can intimidate a significant share of voters, is not systematically related to our dependent variables in any specification and has no effect on our variable of interest. Thus, we refrain from including it. Including it affects none of our results.

[34] Economic Development relates to better access to media and other information sources, and hence a higher probability that voters get to know about their MP's effort. Economically more well off voters are also more likely to be politically interested and concerned with MP performance. One could speculate that retrieving information about parliamentary activity involves more effort and cognitive ability on behalf of the voters than simple *attendance rates*.

5.2 Parliamentary Work

In Table 3, we now turn to the relationship between our main variable of interest and *attendance rates*. *Criminal(a)* has a coefficient of -0.047 which is significant at the 10%-level in column 1, when controlling for state and party dummies only. The coefficient becomes slightly more negative in columns 2 and 3 when we add controls for electoral competitiveness and monitoring intensity, and significant at the 5%-level. This indicates that omitting the two categories leads to a slightly upwardly biased coefficient. The coefficient changes marginally to -0.043 when including candidate characteristics, and remains significant.

The results look rather different when it comes to *parliamentary activity*. As the simple correlation has suggested, there is no systematic relationship between *Criminal(a)* and *parliamentary activity* in columns 5 and 8. The coefficient is negative in columns 7 to 9, however, it fails to be significant in any. Thus there seems to be on average no significant difference between MPs of different types with regard to *parliamentary activity*. This demonstrates that contrary to public opinion, criminal MPs do not necessarily exhibit less effort in all dimensions. This is in line with our model which demonstrated that whether criminal MPs choose less effort depends on the circumstances and other model parameters. An additional intuitive explanation could be that activity has additional unmeasurable private benefits to both types of MPs, like, for example, the utility derived from attention and the spotlight when actively speaking in front of the parliament.

Using our alternative and more stringent measure *Criminal(b)*, which should alleviate measurement error problems in identifying criminal types, confirm and strengthen the existing results. For *attendance rates*, the coefficient for *Criminal(b)* increases in absolute size from about 4% to about 12% lower *attendance rates* for those with a criminal background (column 9-12). This effect is significant at the 1% level. Similarly, the coefficient for *parliamentary activity* becomes more negative, but is still relatively far from being significant (column 13-16). These two results would be in line both with measurement error in identifying the criminal types and a story where the severity of criminal background relates to bigger differences between criminal and non-criminals. While we cannot fully resolve this issue, tests using the number of crimes and its square instead does not support the existence of a non-linear relationship. In conclusion, we find a generally negative and significant relationship between *Criminal* and *attendance rates*, and a

negative but insignificant relationship to *parliamentary activity*.

We further want to test whether the effect of *Criminal* is moderated by monitoring (economic development and literacy rate), as suggested by our theoretical model, and competitiveness (party stronghold and winning margin). We hypothesized that a high degree of information and hence monitoring of MP activity has a moderating effect, as a criminal MP who would choose less effort might not do so when the negative impact she has to fear on her chances for re-election is sufficiently high. Competitiveness on the other hand should not have a significant moderating effect, as it affects criminals and non-criminals alike. Economic development proxies for both access to media and information about candidate performance, and for the average voter's interest in MP effort. Negative consequences of shirking for the MP are more likely in constituencies with a high degree of monitoring, i.e., where voters are better informed and more interested in their MP's performance. To test these hypotheses, we interact *Criminal(a)* with party stronghold, winning margin, economic development and the literacy rate.

Table 4 shows the coefficients for *Criminal(a)* and the respective interaction. All other variables are included but not displayed in the table. The results for both dependent variables show no significant interaction effects with party stronghold and margin (2004), as expected, but also none for literacy rate. However, the interaction effects between economic development and *Criminal(a)* are positive and significant at the 1%-level for both *attendance rates* and *parliamentary activity*. Drawing on our model, one explanation is better monitoring in the more developed constituencies. An alternative explanation within our model framework, which would be supported by anecdotal evidence, is that part of an MP's utility can come from rent extraction (cf. Fisman et al., 2014 and Kapur and Vaishnav, 2011). If more developed constituencies offer better rent-seeking opportunities for criminal MPs, this increases the utility from re-election, and can also narrow the effort gap between criminal and non-criminal types.

Figures 2 and 3 illustrate the moderating effects. The y-axis displays the effect of *Criminal(a)* on effort with its 95% confidence-interval conditional on economic development, which is plotted on the x-axis. In addition, we plot the 95% confidence interval. The marginal effect of *Criminal(a)* is negative and significant for low (*parliamentary activity),* respectively low and median levels *(attendance rates)* of economic

development. These are constituencies where intense monitoring of MPs' effort and access to such information is limited, with on average less resources and wealth. For highly developed constituencies there is no significant difference between MPs with and without criminal charges, in line with our model.

The insignificance of the interaction with literacy rates, which also proxies for monitoring, suggests that rent-seeking rather than monitoring is the more likely explanation. Further tests revealed that the interaction with economic development remains significant, even when controlling for literacy rates, while the interaction with literacy rates remains insignificant even when omitting economic development. Thus, our at first sight counter-intuitive result could be understood in a way that more rent-seeking opportunities, usually considered a negative feature, seem to alleviate the negative relation between an MP's criminal background and their parliamentary work.

5.3 Member of Parliament Local Area Development Scheme (MPLADS) utilization rate

Now we turn to *MPLADS utilization*, which as Keefer and Khemani (2009) argue offers several advantages as a measure of MP effort. The baseline model specification is identical to the one for the first two indicators, except that standard errors are now clustered at the state level to allow for arbitrary correlation within states. As implementation of the project depends on the state bureaucracies, correlation within states is most likely. Our results are, however, unaffected by alternatively clustering on parties. Columns 1-4 in Table 5.1 show the results for *Criminal(a)* and columns 5-8 for *Criminal(b)*. Columns 1 and 4 use only party dummies, columns 2 and 5 add the electoral competitiveness controls, columns 3 and 6 the monitoring intensity controls, and columns 4 and 8 the candidate characteristics.

For our variables of interest, the coefficient on *Criminal(a)* is negative, but remains insignificant in columns 1-4. The coefficients barely change when adding the controls, becoming slightly more negative in column 4 compared to column 1. Thus,we cannot reject the hypothesis that candidates with criminal charges against them generally perform equally well in terms of making use of the development fund scheme. As mentioned above, one concern about these results, however, could be whether the existence of any charge

correctly identifies criminal types of MPs. We again use our *Criminal(b)* measure to alleviate these concerns. Columns 5-8 show the results when using the alternative measure. *Criminal(b)* is related to lower utilization rates in all specifications, significant at the 5%-level. This holds when adding the controls: the coefficient becomes slightly more negative from -7.436 in column 1 to -7.723 in column 4. The coefficient translates to about 7.5 percentage points lower cumulative utilization rates over the legislative period.[35]

The obvious question is whether this relationship has a causal interpretation, or if the coefficient is biased upwards or downwards. In our model with two types of fixed effects, identification relies mostly on within-state, within-party variation. Hence, the results should not be driven by the geographic or political distribution of criminal MPs. Nonetheless, the coefficient might be biased if there are unobserved variables that vary within states or parties and are related to characteristics that affect MPLADS spending. One possibility is, for example, that constituency-specific characteristics like differences in the level of economic development make it easier (or harder) to utilize available MPLADS funds. We can compare the results with and without control variables to get a first indication of the direction of a potential bias. The idea is similar to Altonji et al. (2005): we use selection on observables to assess the effects of selection on unobservables. When adding controls X_i, i.e., controlling for selection on constituency and candidate observables, the coefficient becomes slightly more negative in column 4 compared to column 1 of Table 5.1. This shows first that the coefficient is barely affected by observable selection effects. In addition, if omitted variables bias in terms of selection on unobservables works in the same direction as selection on observables, the negative coefficient will be an upper bound estimates of the true causal effect.

Let us briefly elaborate on this argument. Assume the true regression is $Ef_i = \beta_0 + \beta_1 Cr_i + \beta_2 X_i^* + \varepsilon_i$. If we estimate unconditionally $Ef_{i,s} = b_0^U + b_1^U Cr_{i,s} + \varepsilon_{i,s}^U$ instead, where the superscript U stands for the unrestricted model, our coefficient is biased: $b_1^U = \beta_1 + \beta_2 \frac{Cov(C,X^*)}{Var(C)}$, where the second term indicates Omitted Variable Bias (OVB). Now assume our proxies for the three categories that are contained in the matrix of control variables X_i do not capture the true X_i^*, but $X_i = X_i^* + u_i$. For example, it is reasonable to assume that our proxy for economic development is a noisy measure of the true development level. The restricted model with the controls then is $Ef_i = b_0^R + b_1^R Cr_i + b_2^R X_i + \varepsilon_i^R$. We know

[35] *Criminal(b)* seems to succeed in identifying criminal types in general, alternative specifications using the number of criminal charges and a quadratic term show no indications of a non-linear effect.

that measurement error does not affect the covariance of X_i with Cr_i, but it will underestimate the relation between X_i and Ef_i, hence $b_2^R < \beta_2$. Accordingly, positive OVB ($b_2 \frac{Cov(C,X)}{Var(C)} < \beta_2 \frac{Cov(C,X^*)}{Var(C)}$) occurs for $\beta_2 > 0 \wedge Cov(C,X) > 0$ and negative OVB ($b_2 \frac{Cov(C,X)}{Var(C)} > \beta_2 \frac{Cov(C,X^*)}{Var(C)}$) for $\beta_2 < 0 \vee Cov(C,X) > 0$).

How does this help our interpretation? Generally, we are less concerned with OVB when it works against the direction of our estimated coefficient, i.e., a positive bias with a negative coefficient and vice versa. In the case of MPLADS, our $\beta_1^U < 0$ (column 1), and $\beta_1^R < \beta_1^U$ (column 4), which suggests a positive bias. We can now deduce that even if our empirical proxies only capture the underlying parameters with random measurement error, it holds that $b_1^U < b_1^R < \beta_1 \; \forall \; \beta < 0, b < 0$, i.e., the negative coefficient b_1^R that we report is an upper bound estimate. The assumptions in this consideration are that we have indeed identified the relevant categories in our theory, and that measurement error is random.[36] If our model failed to identify the relevant control categories, selection on other unobservable factors could still be relevant. For this reason, we will conduct further robustness tests for all dependent variables in the next section.

Another way to assess omitted variables is to add the cumulative utilization rate in the previous period to the equation. As constituency delimitation did not change between 1999 and 2004, this lagged dependent variable should capture time-invariant omitted factors, i.e., work similar to a constituency-fixed-effect. The coefficient of *Criminal(b)* in column 1 in the second part of Table 5.2 remains virtually unchanged, giving no indication of such a bias. As another possibility, we consider whether leftover funds from predecessors bias the coefficient. The setup of the MPLADS funds determines that unused resources can be carried over to the next year(s). Higher leftovers increase the overall amount of available money and could bias the measured degree of utilization upwards in cases where there are systematic differences across constituencies. While theoretically plausible, this does either not play a large role in reality, or does not vary systematically between candidates with and without criminal charges, as the coefficient again remains nearly unchanged in size and significant at the 5%-level (column 2).

[36] The argument holds for a negative estimated coefficient even with systematic measurement error as long as β_2 and $Cov(C,X)$ do not change signs. Even if X systematically under- or overestimates X^*, it follows from $b_1^R < b_1^U$ that $b_1^U < \beta_1 \; \forall \; \beta, b < 0$. Accordingly the negative b_1^U is the upper bound estimate. b_1^U is also negative and significant in the case of *Criminal(b)*, which suggests that the true effect is negative as well. For simplicity other covariates were disregarded here; however, their inclusion would (under standard assumptions) not affect the results . The argument cannot be applied for the estimated coefficients on *attendance rates* and *parliamentary activity*, where selection on observables does not clearly indicate a direction of OVB

In addition, we follow the robustness checks in Keefer and Khemani (2009) and test whether political and social fragmentation, as well as electoral volatility in the constituency influence the coefficient (columns 3-5). The original data source for the first two measures is Banerjee and Somanathan (2007). They argue that political fragmentation may reflect greater electoral competitiveness, which as we derived in our model can affect MPs' effort. The measure is defined as $1 - \sum_{i=1}^{N} \mu_{i,C}^2$, where μ_i is the vote share of the i^{th} political party contesting election from the constituency, which is then averaged over the 1991, 1996, 1998 and 1999 elections. Social fragmentation might be relevant if it reduces the provision and changes the composition of local public goods within constituencies. We use a measure of caste and religious fragmentation, based on the census of 1991. Keefer and Khemani (2009) also argue that electoral volatility can be related to MP behavior, as in constituencies where voters are prone to greater shocks and a more unsecure environment, the return to effort by MPs should be lower. As in their study, we use a measure from Nooruddin and Chhibber (2008) that defines volatility as $\frac{1}{2}\sum_{i=1}^{N} \mid \mu_{i,t} - \mu_{i,t-1} \mid$, i.e., the sum of the changes in vote shares of N political parties. This is again averaged over the four previous elections. All three measures might lead to omitted variable bias if they are related both to MPLADS spending and to the likelihood of having a criminal MP. However, this does not seem to be the case. The coefficient of *Criminal(b)* remains negative, nearly unchanged in size, and significant in all specifications (column 2-6). Even when we control jointly for all three variables from Keefer and Khemani (2009), the cumulative utilization rate in the previous period and leftover funds from predecessors, the coefficient remains stable and significant at the five percent level. *Criminal(b)* is related to about 7.6% lower utilization rates.

It can be seen in the Violinplots for all dependent variables in Appendix B, Figure 1 that the distribution of the utilization rate exhibits some potential outliers in its right tail. To make sure these do not distort our results, column 7 of Table 5.2 drops the ten constituencies with the highest utilization rates that constitute this tail. The coefficient becomes somewhat smaller in absolute size, but remains significant. Finally, we compare *Criminal(b)* only to the MPs without any charge at all, i.e., those who are most likely not of the criminal type. As we would expect, this leads to a larger negative coefficient which also remains significant at the 5%-level. Thus, we conclude that there is a negative relationship between criminal background and development fund utilization, which is significant for those MPs with at least two criminal charges, and

unlikely to be explained by selection or omitted variable bias.

5.4 Identification of causal effects

This section discusses whether our estimated effects capture the causal effect of criminal type, by incorporating additional methods for all dependent variables. First, we employ propensity score matching techniques as an alternative estimator to examine whether this affects our results. Second, we analyze whether the results are driven by extreme values or outliers. The results in Table 6 mostly confirm our above result, however they suggest a stronger negative relationship with criminal background.

So far, we have relied on a regression framework to examine our hypotheses, while matching criminal to non-criminal MPs seems to be an intuitive alternative to assess the treatment effect on the treated. Angrist and Pischke (2008) argue that OLS regressions are a natural starting point for empirical studies. Propensity score matching has advantages but requires many somewhat arbitrary choices which can greatly affect results; and in cases where both are consistent OLS is more efficient. Using matching as a robustness check has two advantages. First, it allows us to compare our regression estimates to those from matching the MPs with criminal background (treatment group) to those without (control group). This is interesting as the weights differ between the two estimators: OLS assigns the highest weights to the observations with the largest conditional variance of the treatment status, whereas matching assigns the highest weight to those observations that are most likely to be treated. Second, we can use matching diagnostics to examine how well the treatment and control group are matched to assess the reliability of our estimates.

We use nearest-neighbor (NN) matching with the Mahalanobis distance-metric and robust standard errors (Abadie and Imbens, 2009). As NN-matching estimators were shown to be inconsistent when matching more than two continuous covariates, we use the consistent bias-corrected estimator as outlined in Abadie and Imbens (2006, 2011), which uses a linear function of all covariates as a correction term. We show results for the average treatment effect on the treated both when matching to the two or three nearest neighbors. In our case the choice of three offers the lowest median bias in covariate balancing. Covariate balancing seems to be achieved overall: There are no significant differences in the means of any covariate

except education which is higher for non-criminal candidates (details in Appendix B, Figure 4). If higher education would be related to easier usage of MPLADS funds, for example, this could affect our estimates. However, matching exactly on education level (results available on request) does not alter any of our results.

In a nutshell, the results using matching estimators confirm the regression results both in direction and significance. Column 1 in Table 6 shows that the negative relationship for both *Criminal(a)* and *Criminal(b)* with *attendance rates* becomes stronger but similar in size to the regression results and is significant at least at the five percent level in all specifications. Column 2 confirms that *parliamentary activity* is not generally affected by criminal background. Column 3 for *MPLADS utilization* points in the same direction: the estimated coefficients become more negative. With matching, the negative coefficient of *Criminal(a)* becomes significant at conventional levels when matching on the two nearest neighbors. *Criminal(b)* remains significant, now at the 1%-level, with a more negative coefficient that is again more negative than *Criminal(a)*.

We have already used selection-on-observables to argue why selection bias is less of a concern for the relationship with MPLADS utilization under relatively mild assumptions. Due to the different direction of selection-on-observables, the same argument does not hold for the first two dependent variables. If we do not fully capture the difference in competitiveness and monitoring between those constituencies with and without a criminal MP, unobserved factors could affect the estimates for *attendance rates* and *parliamentary activity*. Theoretically, we would want an instrument that affects the treatment, i.e., the selection of a criminal MP, but is not related to MP effort. One possible instrument is to use the existence of other criminal candidates in the same constituency in the 2004 election. Dutta and Gupta (2012) find that the fielding of such candidates by other political parties attenuates the stigma associated with criminal background. This would not directly affect incumbent effort if the criminal candidates were not relevant for the final outcome of the election. A crude test of exogeneity shows that it is not significant in the main equation conditional on the other variables in X_i. The instrument would be significant in the first stage, but the F-statistics are comparatively small and the Kleibergen-Papp rk LM and F-statistics do not

confirm the validity of the model. This is why we refrain from using an IV strategy.[37]

Instead, we further approach potential selection issues by using so called endogenous binary-variable models (treatment effect models). The approach of these Heckit-models is similar to Heckman selection-models: The selection problem is approached by explicitly modeling selection instead of only proposing a supposedly exogenous instrument. Treatment effect regression differs from sample selection models as the dummy treatment variable is directly entered in the regression equation and the outcome variable is observed for both the treated and the untreated subjects. The advantage of this potential outcome model is that it provides information about the effects of non-linear selection bias.

Specifically, we model two equations. Our simplified regression equation is $Ef_i = Cr_i b_1 + X_i^{'} b_2 + \varepsilon_i$ where X_i contains the controls and fixed effects and Cr_i is the dummy treatment indicator. Our probit selection equation estimates the latent variable $Cr_i^* = Z_i^{'}\nu + u_i$, with

$CR_i = \left\{ \begin{array}{ll} 1 & \text{if } CR_i^* > 0, \\ 0 & \text{if } CR_i^* \leq 0 \end{array} \right\}$ and $Prob(Cr_i = 1 \mid Z_i) = \emptyset(Z_i^{'}\nu)$, respectively, $Prob(Cr_i = 0 \mid z_i) = 1 - \emptyset(Z_i^{'}\nu)$. Z_i is a row vector of variables determining the selection process and ε_i and u_i are assumed to be bivariate normal with zero mean and covariance matrix $\left(\begin{array}{cc} \sigma_\varepsilon & \rho \\ \rho & 1 \end{array} \right)$. $\rho \neq 0$ reflects the assumed endogeneity of the treatment, and $\sigma_u^2 = 1$ for identification.

This is a switching regression depending on whether $Cr^* > 0$ or $Cr^* < 0$, with separate forms for the outcome under treatment ($Ef_{i,g} = (Z_{i,g}^{'}\nu + u_{i,g})b_1 + X_i^{'} b_2 + \epsilon_{i,g}$) or non-treatment ($Ef_{i,g} = X_i^{'} b_2 + \epsilon_{i,g}$) regime. For a more detailed description see for example Cameron and Trivedi (2005, sec. 16.7 and 25.3.4)

[37] Chemin (2012) suggests a regression discontinuity design as an alternative, where he focused on cases where a criminal contested against a non-criminal. We do not use RDD in our main specification for several reasons. First, while the treated and control groups seem to be balanced within a +/-5% vote score difference, the assumption of continuous density in the neighborhood of the discontinuity is rejected by the McCrary-test (McCrary, 2008). Specifically, criminal candidates seem to win close elections much more often than chance would predict, as indicated by the higher density to the right of the discontinuity. This apparent score manipulation makes us skeptical about the use of RDD here. Moreover, the number of close races between winner and runner-up is very limited. If we use an already wide bandwidth of 10 (20) percent vote difference, we are left with 31 (62) observations. The interested reader can find the related graphs in Appendix B, Figure 5 and 6. Graphically, one can spot an obvious discontinuity with regard to *attendance rates* and potentially for *parliamentary activity*: Using a simple specification with a regular and quadratic score variable, the difference between Criminals and Non-Criminals becomes more negative compared to our main model and remains significant at the 5%-level for *attendance rate*. *Parliamentary activity* becomes negative and significant as well.

and Maddala (1983). We conduct the estimation using full maximum likelihood under a normal distribution assumption.[38]

We do not claim that this approach resolves all potential selection/omitted variables bias concerns, and it relies on assumptions about the correlation structure. Rather, we regard it as a further useful robustness check to assess the sensitivity of our results to changes in the econometric specification and the direction of selection-bias. The z_i in the selection equation contains all variables in x_i, plus the variable "other criminals" which counts the number of additional criminal candidates in the constituency in 2004. As reported above, other criminals did not pass the specification tests in IV regressions, but it provides plausibly exogenous variation about the selection of a criminal candidate and could work satisfactorily here. We run the regressions for all three dependent variables. Lamda is the inverse mills-ratio or non-selection hazard, and the parameter rho indicates the correlation between the error terms ε_i and u_i. We test the model assumption with a likelihood ratio test of an independent probit and regression model versus the treatment effect likelihood, a test of rho=0 that is Chi-square distributed. The test statistics rejects that rho equals zero for *attendance rate* and *MPLADS utilization*, indicating that these models are valid. The coefficient for other criminals is positive and significant in the selection equation as predicted (Table 6 and Appendix B, Table 6).

The results further support our earlier impression that not controlling for selection effects biases the OLS coefficient rather upwards than downwards. The negative relationship between criminal background and *attendance rates* becomes more negative and significant at the 1%-level. The same holds for the relation to *MPLADS utilization*: The coefficient of *Criminal(a)*, which was negative but insignificant in the baseline model, becomes larger in size and significant at the 1%-level. The next rows omit potential extreme values or outliers in the earlier regression specifications in Tables 3 and 4. First, we omit the observations with the largest values of the respective dependent variable. Second, we calculate the residuals of the full regression, and omit the observations with the one-percent largest positive and negative residuals. The results for all dependent variables and both *Criminal(a)* and *Criminal(b)* are unaffected, indicating that

[38] Alternatively we can regard this model as a non-standard Maximum likelihood estimator. The likelihood function $L_N(\Theta) = f(y, X|\Theta) = f(y|X, \Theta)f(x|\Theta)$ generally would require specifying the conditional density of y given X as well as the marginal density of X. It is standard to use only the conditional density $f(y|X, \Theta)$, and ignore $f(X|\Theta)$. This in essence assumes exogenous sampling and conditional independence. Treatment effects models drop this assumption, but instead assume a specific correlation structure of the error terms of the two equations to be estimated.

the results are not driven by outliers or few observations.

Finally, we want to demonstrate how likely it is that, if all our prior robustness tests which suggest an upward bias failed, our results are explained by selection-on-unobservables. While our attempts so far suggest that selection, if anything, biases against the negative coefficient we measure, we cannot rule out that there are unobservable factors that lead to a problematic bias in the direction of our effect. Thus, we demonstrate with techniques developed in Altonji et al. (2005) how much larger on average selection bias on unobserved factors would have to be compared to selection on observed factors to fully explain our results.

The strategy is to use selection on observables to assess the severity of potential selection bias for the results. We compare two kinds of regressions: first, one without controls (U_1 = unrestricted) to one with our full set of controls (R = Restricted); and second, one with a limited set of controls for fixed effects (U_2) to one with full controls (R). We then calculated a "Selection ratio" (SR), which is the necessary ratio of selection on unobservables to observables to fully explain our coefficients as $\hat{\beta}_R/(\hat{\beta}_U - \hat{\beta}_R)$. The denominator, i.e., the difference between the $\hat{\beta}$ coefficients indicates the degree to which our estimate is affected by selection on observables. A small difference indicates little selection effects. $\hat{\beta}_R$ in the nominator enters positively in the ratio, as we need stronger selection on unobservables to explain a larger coefficient. Altonji et al. (2005) provide the underlying assumptions and Bellows and Miguel (2008) a formal derivation.

While our empirical proxies might not perfectly capture the theoretical parameters, they are comprehensive and should be a useful guide to assess selection-on-unobservables. Altonji et al. (2005) posit that "there are strong reasons to expect the relationship between the *unobservables* and (...) generally any potentially endogenous treatment to be weaker than the relationship between the observables and dependent". The bottom part of Table 6 shows the respective ratios for $\frac{\hat{\beta}_R}{(\hat{\beta}_{U1}-\hat{\beta}_R)}$ and $\frac{\hat{\beta}_R}{(\hat{\beta}_{U2}-\hat{\beta}_R)}$, for our two limited sets (U_1) and (U_2). The results strongly confirm the negative relationship between criminal background and *attendance rates*: Selection on unobservables would have to be at least 2.1 - 3.3 [2.5 - 3.7] times as strong as selection on unobservables to fully explain the negative coefficient of *Criminal(a)* [*Criminal(b)*]. To explain the negative relationship between *Criminal*(b) and *MPLADS utilization* rates, selection on unob-

servables would have to be between 11 and 45 times as high as on observables.

Oster (2013) further formalizes and extends these ideas. More specifically, she argues that the extent to which robustness to selection-on-observables confirms our confidence in coefficient stability depends on the degree to which those observables explain variance in the dependent variable. Intuitively, this can be easily understood. We could add additional variables to our regression which are neither correlated with the dependent or our variable of interest. Adding them would not affect our coefficient estimate, however, this would not be very revealing. If additional observable controls explain considerable variation, but do not affect our coefficient by much, we can assume that unobservables are not likely to do so as well.[39] When applying the suggested assumptions our identified coefficients sets do not include zero for both *attendance rates* and *MPLADS utilization*. Thus, these two relations pass this test as well.

6 Concluding remarks

In this paper we examine whether the fact that a member of parliament has a criminal background influences his effort in the parliament and in developing his constituency. To be able to understand the implications of criminality on MP behavior, we developed a model that illustrates the incentives faced by an elected MPs with regard to their executed effort. The model incorporates voters' monitoring intensity with regard to parliamentarians' efforts, as well as the competitiveness and other characteristics of their constituencies, to make predictions about the effect of criminal background on individual effort. We show that it is not obvious that criminal MPs always exert less effort, but rather that differences crucially depend on the other parameters in the model.

The hypotheses derived from the PA-model are then put to an empirical test using data from the 14th Indian 2004 Lok Sabha election, and the subsequent 2004-2009 legislative period. While criminals in politics are a general issue, in India criminal MPs are a widespread phenomenon and widely regarded as a danger to the functioning of the world's largest democracy. This analysis was made possible by a judgment of

[39] We also need an assumption about the maximum R-squared that can be systematically explained and is not due to pure noise. Oster (2013) suggests to apply the same standard to observational studies that are fulfilled by randomized studies which used control variables and were published in five selected top journals. She calculates that the appropriate R_{max} is 2.2 times theR^2 in the specification with all observable controls. The formula for the identified set boundary is $\beta^* = \widetilde{\beta} - \tilde{\delta} \times \frac{(\dot{\beta}-\widetilde{\beta})\times(R_{max}-\tilde{R})}{(\tilde{R}-\dot{R})}$.

the Indian Supreme Court in 2003 which asked every candidate to provide sworn affidavits that have to include details not only about their personal educational and financial particulars but also about their criminal background. We restrict our analysis to this legislative period because constituency boundaries were changed in the 2009 election. Thus, it is no longer possible to control for important constituency characteristics like the winning margins in previous elections.

We augment the existing literature, which has mostly focused on the initial decision of whether to field a criminal candidate in the first place (Aidt et al., 2015) and on the connection of MP criminality with rather disjointed proxies for MP effort like final consumption in the respective district or constituency as in Chemin (2012) and luminosity as in Prakash et al. (2014). We provide a comprehensive direct assessment of effort by using three measures that each capture a slightly different facet of MP behavior. First, we use *attendance rates* (respectively absenteeism) as for example in Besley and Larcinese (2011), Gagliarducci et al. (2010, 2011), and Mocan and Altindag (2013). Second, we measure MPs' *parliamentary activity* based on the number of questions asked and their participation in debates (similar to Arnold et al., 2014 and Mocan and Altindag, 2013). Third, we follow Keefer and Khemani (2009) and use the utilization rate of the Member of Parliament Local Area Development Scheme (MPLADS) which offers several important advantages as a measure of effort on behalf of the MPs' respective constituencies. We use two proxies for criminal MPs, *Criminal(a)* for all MPs with at least one crime, and *Criminal(b)* only for those with a least two charges.

Our empirical results support the conclusions from our model, but also provide further interesting details. Focusing on the first measure, it seems to be the case that having a criminal background is generally related to higher absenteeism rates. This relation is robust to the inclusion of party and state fixed effects, as well as controls for electoral competition, monitoring intensity and candidate characteristics. On the other hand, results concerning the second measure indicate that there is no obvious correlation between *parliamentary activity* and criminal background. Our model has suggested that differences in effort levels between criminals and other MPs might be partly explained by the potential to extract rents or differences in monitoring intensity. Wealthier constituencies are more attractive for rent extraction and related to better monitoring, which led to the hypotheses that criminal MPs in rich constituencies work

relatively more because they put more emphasis on their reelection prospects. This is exactly what the data show: The difference in effort between criminals and other MPs is particularly pronounced in poor constituencies and narrows in richer areas. Criminal background has a statistically significant negative relation with both *attendance rates* and *parliamentary activity* in less and medium developed constituencies.

With regard to making use of the MPLADS to develop their constituency, criminal background is also related to lower utilization rates. This coefficient is not significant for *Criminal(a)*, however, becomes statistically significant for *Criminal(b)*. This suggests that not all MPs with criminal charges necessarily form a homogenous group: there are some individuals who have been criminal only once or are falsely accused and those that repeatedly broke the law. For the latter it is much more likely that they still engage in criminal activities and can, for example, use bribes or voter intimidation to secure their reelection.

There are potential concerns as to whether our coefficients have a causal interpretation. Omitted variable bias, in our case mostly selection effects, might bias our coefficients. Based on our theoretical considerations, we argue that it is more likely that our point estimates are upwardly rather than downwardly biased. This assumption is supported by a series of robustness checks. For the MPLADS variable, we follow Keefer and Khemani (2009) and run a series of falsification tests to see whether omitted variables like political or social fragmentation are responsible for our results. As expected, including these additional covariates separately or jointly leads to more negative coefficients. Moreover, the relationship is robust to controlling for the utilization rate in the period before, which should capture omitted constituency-specific variables. Specifically for *MPLADS utlization* we use selection on observables to demonstrate that the negative and significant coefficient for *Criminal(b)* constitutes an upper bound for the negative effect of criminal background on effort.

Similarly, we show in a series of more general robustness checks that the results using a matching estimator are quantitatively very similar to the OLS estimates, with on average slightly more negative coefficients. Moreover, the matching statistics suggest a good covariate balance across treatment and control group. Regressions omitting the most influential observations or potential outliers further support the baseline results. Criminal background on average results in higher abseentism and lower utilization rates. *Cri-*

minal(a) relates to on average about 5% lower *attendance rates,* and *Criminal(b)* relates to about 7% lower utilization rates of the MPLADS program. We argue and explain why we think regression discontinuity and instrumental variable designs are invalid or at least not feasible alternatives. Instead we use endogenous binary-variable models that explicitly model the selection process, with the existence of other criminal candidates as an additional exogenous selection-variable. Finally, we draw on the seminal paper by Altonji et al. (2005), and demonstrate that our findings are unlikely to be caused solely by selection bias. Selection-on-unobservables would have to work partly contrary to selection on observables, and its effect would have to be 2 to 21 times stronger to fully explain our results.

Credibility and trust in representatives is of crucial importance for the credibility of India's democracy. While transparency increases reporting of corruption events and corrupt officials (see Vadlamannati and Cooray, 2015) and the provisioning of criminal information to voters should help them make informed choices, a large number of criminal candidates still make it to the parliament. Step by step, evidence shows that there seem to be detrimental consequences to criminals holding public office, and we hope that our study further helps to enhance transparency.

A Appendix. A generalized model

In section 3, we assumed a specific functional relationship between political effort and the fraction of informed voters who end up voting for the incumbent. In this section, we relax this assumption.[40]

Again, consider two incumbents who differ only in the criminal characteristic. As before, the corresponding personal characteristic vectors are denoted by x^c and x^n. Let $\mathcal{C}$ be the characteristic space (i.e. the space consisting of all possible characteristic combinations). In general, a function which represents the fraction of informed voters who end up voting for the incumbent needs to assign a share of voters (i.e. a number from the unit interval) to each characteristic vector $x \in \mathcal{C}$ and each effort level $e \in [0,1]$. Consider such a function $f(e,x)$ where $f : [0,1] \times \mathcal{C} \to [0,1]$. We assume that $f(e,x)$ is differentiable with respect to e, where $f_e > 0$ and $f_{ee} < 0$ for all $x \in \mathcal{C}$ (i.e. the marginal impact of effort on the share of voters is positive, but decreasing). Furthermore, empirical evidence indicates that voters penalize criminality (see section 3). Therefore, a criminal background as a characteristic feature decreases the marginal impact of effort on the fraction that is informed, $f_e(e,x^n) > f_e(e,x^c)$ for all $e \in (0,1)$. It seems plausible that the higher the effort of a criminal MP, the less skeptical the voters will be.In other words, we assume that criminal MPs can partly overcome the scepticism of the voters towards them when engaging in considerable effort. Thus, the difference between marginal impacts of effort $\Delta^m := f_e(e,x^n) - f_e(e,x^c)$ is assumed to decrease in effort, $\Delta^m_e < 0$.

Assuming an interior solution, the first-order condition of optimization problem (1) of an incumbent with characteristic $x \in \mathcal{C}$ is

$$\frac{\iota}{(1-\iota)} \cdot \frac{1}{2(b-a)} \cdot U \cdot f_e(e,x) = 1 \tag{3}$$

where ι, U, a and b are defined as in section 3.

Proposition 3 shows that in the generalized model, the qualitative impact of ι, U and b on incumbent's optimal effort level are the same as in section 3.

[40] We still use the following notation for partial derivatives: $\frac{\partial f}{\partial x} := f_x$ and $\frac{\partial^2 f}{\partial x \partial y} := f_{xy}$.

Proposition 3. The optimal effort level of the incumbent is higher if

(i) voters are better informed (high ι)

(ii) the MP's utility from holding office is higher (high U)

(iii) the expected level of support is lower (low b)

Proof. Note that the optimal effort level of an incumbent with characteristic $x \in \mathcal{C}$ is implicitly given by equation (3). Consider the left-hand side of (3), $l(e, U, \iota, x) := \frac{\iota}{(1-\iota)} \cdot \frac{1}{2(b-a)} \cdot U \cdot f_e(e, x)$, and observe that $l_U, l_\iota > 0$ and $l_b < 0$. Now, recall that $sign(l_y) = sign(e_y^*)$ where $y \in \{\iota, U, b\}$ □

The next proposition examines the difference between the optimal effort levels of a a criminal and a non-criminal incumbent, e_c^* and e_n^*.

Proposition 4. The difference between optimal effort levels $\Delta_{e^*} = e_n^* - e_c^*$ is

(i) strictly positive

(ii) decreasing in ι

(iii) decreasing in U

Proof. (i) The optimal effort levels are implicitly given by (3). Equation (3) can be rewritten as $f_e(e, x) = z$ where $z \equiv \frac{(1-\iota)\cdot 2(b-a)}{\iota \cdot U}$. Thus, the optimal effort levels satisfy $f_e(e_n^*, x^n) = z = f_e(e_c^*, x^c)$. By assumption, it holds that $f_e(e, x^n) > f_e(e, x^c)$ for all effort levels. It follows that $e_n^* \neq e_c^*$. Since, furthermore, f_e is strictly monotone decreasing in e for all $x \in \mathcal{C}$, $f_e(e_n^*, x^n) = f_e(e_c^*, x^c)$ implies $e_c^* < e_n^*$. Hence, $\Delta_{e^*} > 0$. (ii) Proposition (3) shows that optimal effort levels are increasing in ι. At the same time, by assumption, the difference between marginal impacts of effort Δ^m decreases. Consequently, the difference Δ_{e^*} is decreasing in ι. Statement (iii) can be shown similarly. □

Tables and Figures

Table III.1: Descriptive statistics

	Count	Mean	SD	Min	Max	Mean - Normal	Mean - Criminal
Effort measure							
Attendance rate	394	1	0	0.06	0.96	0.73	0.67
Parliamentary activity	394	1	1	0.00	4.38	0.81	0.85
MPLADS utilization	439	106	20	60.50	260.00	106.02	104.45
Criminal Record	439						
Criminal(a)	439	0	0	0	1		
Criminal(b)	439	0	0	0	1		
Electoral competition							
Party stronghold (3time winner)	439	0	0	0	1	0.24	0.23
Margin (2004)	439	1	0	0	1	0.61	0.57
Voter turnout (2004)	439	10	1	6	12	9.80	9.71
Constituency characteristics							
Economic development (log sum of night light intensity)	439	55.69	12	25.86	85.43	56.05	54.55
Literacy rate	439	0.21	0	0.00	1.00	0.22	0.15
PC is reserved for minority SC or ST	439	0	0	0.00	0.61	0.13	0.11
No of voters	439	52	11	26.00	77.00	0.00	0.00
Candidate characteristics							
Candidate age (at election)	439	1.61	0.74	0.00	2.00	52.73	50.38
Formal education in 3 steps	439	1	1	0.00	3.00	1.64	1.50
Experience in parliament =no of times the MP has won before	439	1	0	0.00	1.00	0.77	0.55
Gender	439	16	1	1.61	20.33	0.92	0.94

Notes: Descriptive statistics were calculated for the maximum regression sample size.

Table III.2: Criminals by party affiliation

	Normal		Criminal	
Bharatiya Janata Party	82	[78.8%]	22	[21.2%]
Communist Party of India (Marxist)	33	[82.5%]	7	[17.5%]
Indian National Congress	103	[84.4%]	19	[15.6%]
Rashtriya Janata Dal	11	[52.4%]	10	[47.6%]
Other	106	[69.7%]	46	[30.3%]
Total	335	[76.3%]	104	[23.7%]

Figure III.2: Geographical distribution of candidates with criminal charges across constituencies for the 14th Lok Sabha (2004 national election).

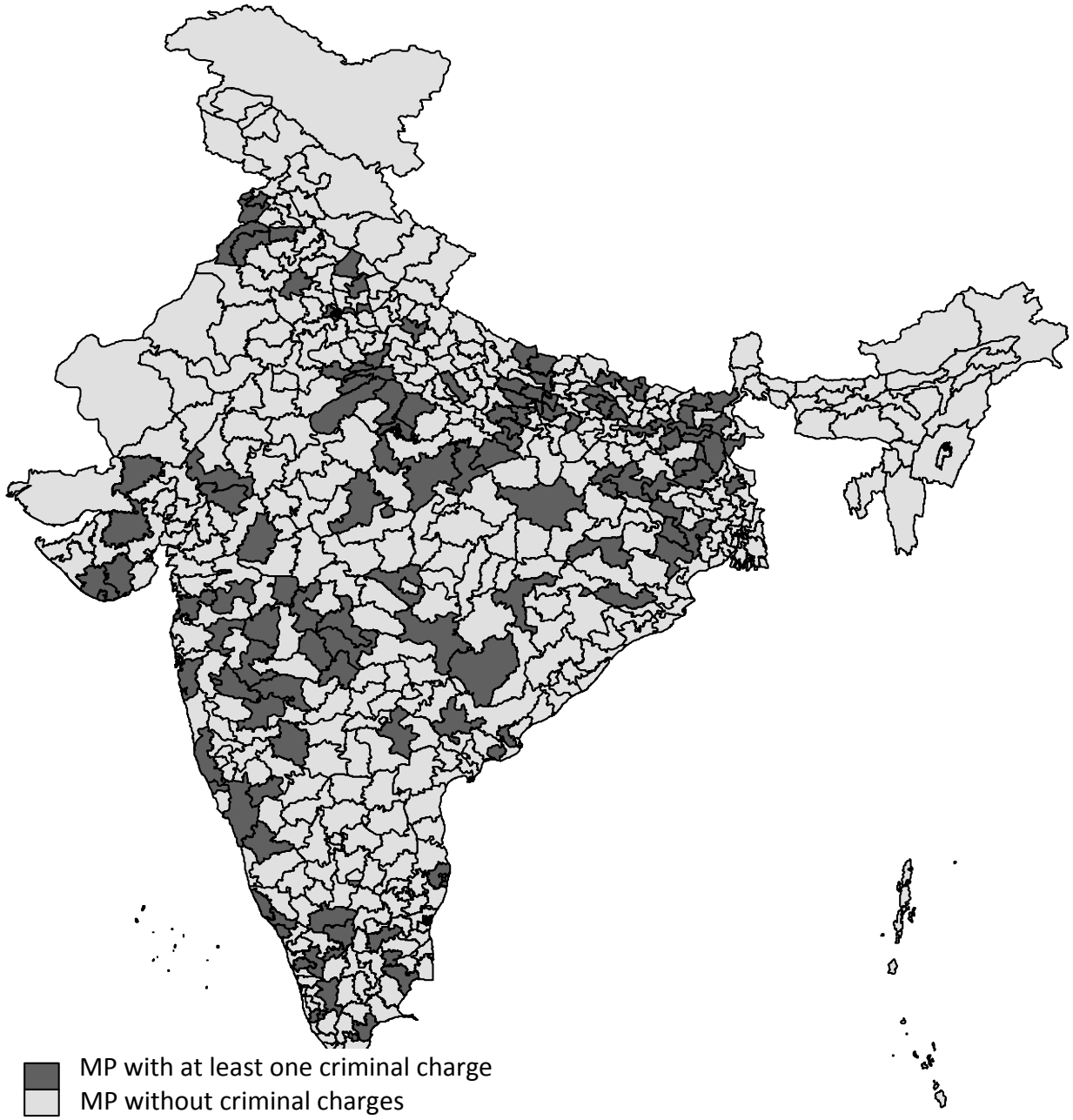


Table III.3: Main results for attendance rates and parliamentary activity

Dependent variable: Attendance rate	(1)		(2)		(3)		(4)	
Criminal(a)	**-0.046***	**[0.025]**	**-0.049****	**[0.022]**	**-0.050****	**[0.020]**	**-0.043***	**[0.023]**
Dep. variable: Parliamentary activity	(5)		(6)		(7)		(8)	
Criminal(a)	**0.002**	**[0.066]**	**-0.006**	**[0.065]**	**-0.003**	**[0.071]**	**-0.006**	**[0.075]**
Number of MPs	394		394		394		394	
State Dummies	Yes		Yes		Yes		Yes	
Party Dummies	No		Yes		Yes		Yes	
Electoral Competitiveness	No		Yes		Yes		Yes	
Monitoring Intensity	No		No		Yes		Yes	
Candidate Characteristics	No		No		No		Yes	
Dependent variable: Attendance rate	(9)		(10)		(11)		(12)	
Criminal(b)	**-0.126****	**[0.051]**	**-0.128*****	**[0.047]**	**-0.126*****	**[0.045]**	**-0.104****	**[0.045]**
Dep. variable: Parliamentary activity	(13)		(14)		(15)		(16)	
Criminal(b)	**-0.180**	**[0.160]**	**-0.181**	**[0.152]**	**-0.174**	**[0.156]**	**-0.150**	**[0.153]**
Number of MPs	394		394		394		394	
State Dummies	Yes		Yes		Yes		Yes	
Party Dummies	No		Yes		Yes		Yes	
Electoral Competitiveness	No		Yes		Yes		Yes	
Monitoring Intensity	No		No		Yes		Yes	
Candidate Characteristics	No		No		No		Yes	

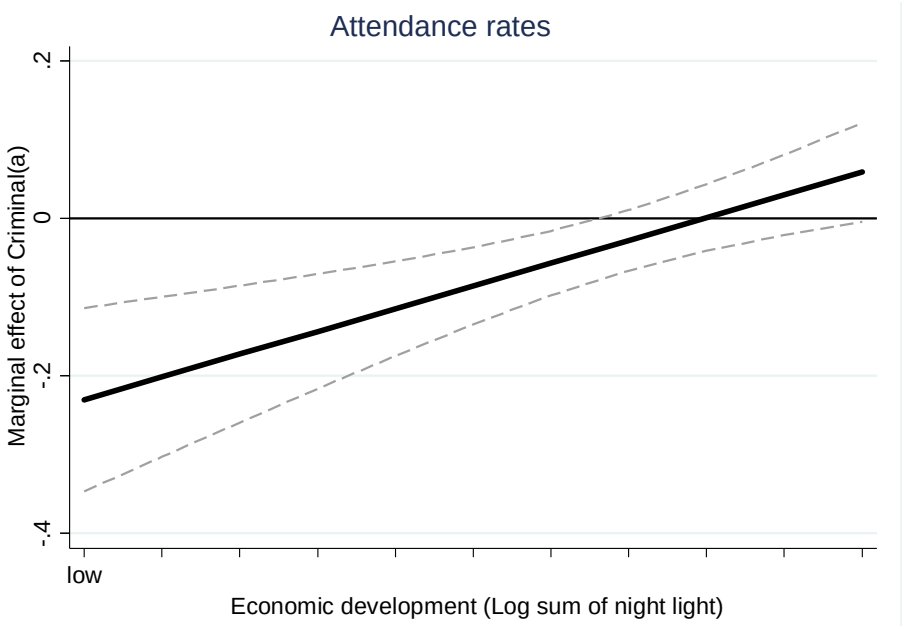
Notes: Dependent variable as specified above over the full legislative period 2004-2009. Standard errors are clustered at the party level. *Criminal(a)* is defined as those having at least one criminal charge against them, *Criminal(b)* as those having more than one criminal charge against them. All regressions include the control variables as specified in Table III.1 as indicated in the respective column. *** (**, *) indicates significance at the 1 (5, 10) percent level respectively.

Table III.4: Interaction effects

Dependent variable: Attendance rate								
Criminal(a)	-0.055***	[0.018]	-0.051**	[0.023]	-0.606***	[0.158]	-0.107	[0.117]
Interaction with:								
Party Stronghold	0.070	[0.065]						
Margin (2004)			0.077	[0.199]				
Economic Development					0.058***	[0.016]		
Literacy rate							0.001	[0.002]
Number of MPs	394		394		394		394	
Dependent variable: Parliamentary activity								
Criminal(a)	0.035	[0.081]	0.029	[0.068]	-1.699***	[0.421]	0.12	[0.345]
Interaction with:								
Party Stronghold	-0.148	[0.111]						
Margin (2004)			-0.428	[0.744]				
Economic Development					0.194***	[0.058]		
Literacy rate							0.001	[0.008]
Number of MPs	392		392		392		392	

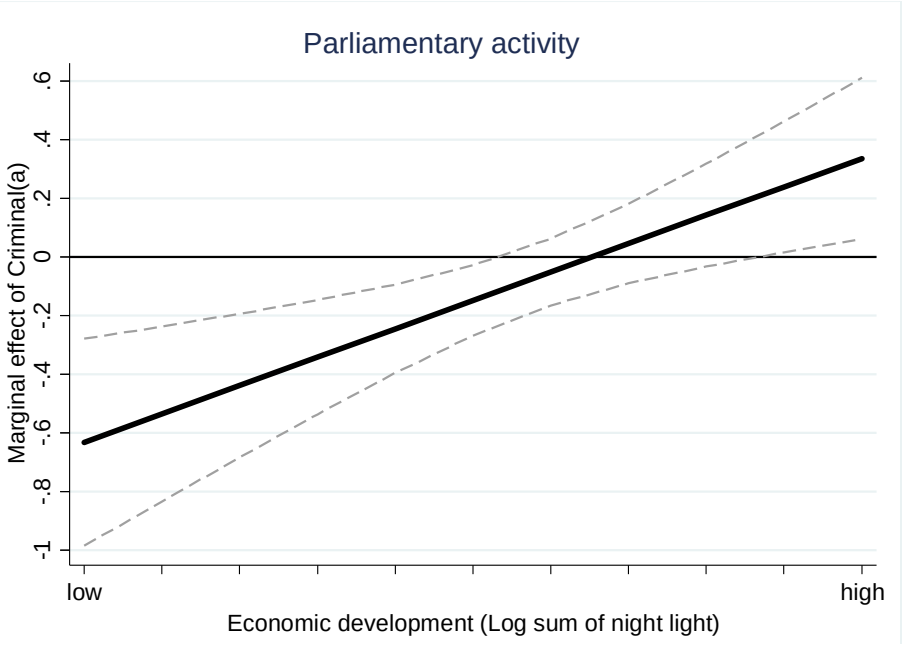
Notes: Dependent variable as specified above over the full legislative period 2004-2009. All regressions include all control variables as specified in Table III.1, including dummies for major states and parties. *Criminal(a)* is defined as those having at least one criminal charge against them. Standard errors [in brackets] are clustered at the party level. *** (**, *) indicates significance at the 1 (5, 10) percent level respectively.

Figure III.1: Marginal Effect of Criminal(a) on attendance rates conditional on economic development



Notes: Marginal effect of a *Criminal(a)* dummy for different levels of economic development. Dotted lines represent the 95% confidence intervals.

Figure III.2: Marginal Effect of Criminal(a) on parliamentary activity conditional on economic development



Notes: Marginal effect of *a Criminal(a)* dummy for different levels of economic development. Dotted lines represent the 95% confidence intervals.

Table III.5.1: Main results for Member of Parliament Local Area Development Scheme (MPLADS) utilization

	(1)		(2)		(3)		(4)	
Criminal(a)	**-3.014**	**[3.519]**	**-3.302**	**[3.563]**	**-3.273**	**[3.601]**	**-3.419**	**[3.733]**
	(5)		(6)		(7)		(8)	
Criminal(b)	**-7.083****	**[2.677]**	**-7.372****	**[3.005]**	**-7.042****	**[3.106]**	**-7.723****	**[3.415]**
Number of MPs	439		439		439		439	
State dummies	Yes		Yes		Yes		Yes	
Party dummies	No		Yes		Yes		Yes	
Competition controls	No		Yes		Yes		Yes	
Monitoring controls	No		No		Yes		Yes	
Candidate characteristics	No		No		No		Yes	

TableIII. 5.2: Robustness tests (based on column 4)

Criminal(b)	**Coef.**	**-7.793***	**-7.658****	**-7.479****	**-9.664****	**-7.679****	**-7.643****	**-5.642***	**-8.541****
Additional controls for	**SE**	**[4.050]**	**[3.554]**	**[3.355]**	**[3.482]**	**[3.529]**	**[3.506]**	**[3.015]**	**[3.676]**
Development fund utilization (1999-2004)		Yes					Yes		
Leftover funds from predecessor			Yes				Yes		
Political fragmentation 1991- 1999				Yes			Yes		
Caste and religious fragmentation					Yes		Yes		
Electoral volatility						Yes	Yes		
Sensitivity analysis									
Omit 10 constituencies with highest utilization rates								Yes	
Only Criminal(b) vs. Non-Criminals									Yes

Notes: Robustness checks. Dependent variable is the cumulative utilization rate over the 2006-2009 period . *Criminal(a)* is defined as those having at least one criminal charge against them, *Criminal(b)* as those having more than one criminal charge against them. All regressions include the control variables as specified in Table III.1 as indicated in the respective column. Standard errors [in brackets] are clustered at the state level. *** (**, *) indicates significance at the 1 (5, 10) percent level respectively.

Table III.6: Robustness checks for all dependent variables

Dependent variable	Attendance rate		Parliamentary activity		MPLADS utilization	
Baseline results	Coef.	SE	Coef.	SE	Coef.	SE
Criminal(a)	-0.043*	[0.023]	-0.006	[0.075]	-3.419	[3.733]
Criminal(b)	-0.104**	[0.045]	-0.150	[0.153]	-7.723**	[3.415]
	Matching estimator (nearest neighbor matching)					
Criminal(a) & NN (2)	-0.061***	[0.023]	-0.048	[0.136]	-7.077**	[3.283]
Criminal(a) & NN (3)	-0.054**	[0.022]	0.003	[0.125]	-4.291	[2.865]
Criminal(b) & NN (2)	-0.135***	[0.046]	-0.317	[0.205]	-18.169***	[6.095]
Criminal(b) & NN (3)	-0.135***	[0.043]	-0.331	[0.208]	-12.222***	[4.642]
	Treatment effect estimator					
Criminal(a)	-0.187***	[0.069]	-0.211	[0.172]	-10.659***	[3.655]
Lamda	0.09		0.12		4.28	
Rho	0.57		0.16		0.22	
Prob > Chi^2	0.0744		0.1183		0.004	
Regressions	*w/o 2% largest values of dependent variables*					
Criminal(a)	-0.046*	[0.024]	0.005	[0.072]	-2.155	[2.792]
Criminal(b)	-0.101**	[0.045]	-0.034	[0.086]	-5.699*	[2.907]
	w/o 1% largest positive and negative residuals					
Criminal(a)	-0.042*	[0.022]	-0.034	[0.086]	-3.472	[2.811]
Criminal(b)	-0.113**	[0.042]	-0.137	[0.136]	-6.666**	[3.138]
	Using selection-on-observables to assess the bias from unobservables					
Controls (Restricted/ Full))	Selection ratio SR=$ß_R/(ß_U-ß_R)$					
Criminal(a)	Identified ß-set	SR	Identified ß-set	SR	Identified ß-set	SR
None (U_1) / Full controls (R)	[-0.010, -0.043]	3.3	[-0.006,-0.010]	1.5	[-3.419, -3.920]	11.4
Fixed effects (U_2) / Full controls (R)	[-0.010, -0.043]	3.7	[-0.006, 0.000]	6.4	[-3.419, -4.140]	17.2
Criminal(b						
None (U_1) / Full controls (R)	[-0.030, -0.104]	2.1	[-8.680, -0.150]	1.6	[-7.723, -8.680]	20.1
Fixed effects (U_2) / Full controls (R)	[-0.080, -0.104]	2.5	[-9.810, -0.150]	7.8	[-7.723, -9.810]	44.6

Notes: Matching was conducted on all variables that acted as controls in the prior regressions, including party and state dummies. The appendix shows balance statistics. The treatment effect regressions are estimated using maximum likelihood. In the first row, selection is based on all constituency characteristics from the baseline model. For the regression in the bottom part, we first calculated the baseline regression. Then we calculated the observations with the largest residuals and omitted them from the regressions.
The selection ratio SR is further explained and derived in Altonji et al. (2005). The ratio indicates how much larger unobservables would have to influence the coefficient until it converges to zero. The identified set is explained in Oster (2014). It contains the range of ß-estimates under the assumption of proportional selection on un- and observables, and a maximum R-squared comparable to the standards fulfilled by randomized studies.

IV References

Abbas, S. M. Ali, Belhocine, Nazim, ElGanainy, Asmaa A., & Horton, Mark A. 2010. *A historical public debt database.* IMF Working Paper No. 10/245, Washington, DC: International Monetary Fund, Available at http://www,imf,org/external/pubs/cat/longres,cfm?sk=24332,0.

Abadie, Alberto, & Imbens, Guido W. 2006. Large sample properties of matching estimators for average treatment effects. *Econometrica*, **74**, 235–267.

Abadie, Alberto, & Imbens, Guido W. 2009. *Matching on the estimated propensity score*. NBER Working Paper No. 15301.

Abadie, Alberto, & Imbens, Guido W. 2011. Bias-corrected matching estimators for average treatment effects. *Journal of Business and Economic Statistics*, **29**, 1–11.

Afonso, António. 2003. Understanding the determinants of sovereign debt ratings: Evidence for the two leading agencies. *Journal of Economics and Finance*, **27**(1), 56-74.

Afonso, António, Furceri, Davide, & Gomes, Pedro. 2012. Sovereign credit ratings and financial markets linkages: Application to European data. *Journal of International Money and Finance*, **31**(3), 606-638.

Ahearne, Alan G., Griever, William L., & Warnock, Francis E. 2004. Information costs and home bias: An analysis of U.S. holdings of foreign equities. *Journal of International Economics*, **62**(2), 313-336.

Aidt, Toke, Golden, Miriam A., & Tiwari, Devesh. 2015. *Incumbents and criminals in the Indian National Legislature*. mimeo.

Alesina, Alberto, Devleeschauwer, Arnaud, Easterly, William, Kurlat, Sergio, & Wacziarg, Romain. 2003. Fractionalization. *Journal of Economic Growth*, **8**(2), 155-194.

Alsakkaa, Rasha, & ap Gwilym, Owain. 2010. Leads and lags in sovereign credit ratings. *Journal of Banking & Finance*, **34**(11), 2614-2626.

Altonji, Joseph G., Elder, Todd E., & Taber, Christopher R. 2005. Selection on observed and unobserved variables: Assessing the effectiveness of catholic schools. *Journal of Political Economy*, **113**, 151–184.

Ammer, John, & Packer, Frank. 2000. *How consistent are credit ratings? A geographic and sectoral analysis of default risk*. International Finance Discussion Papers Number 668, Washington, DC: Board of Governors of the Federal Reserve System.

Angrist, Joshua D., & Krueger, Alan B. 1999. Empirical strategies in labor economics. *Handbook of Labor Economics*, 1277–1366.

Angrist, Joshua D., & Pischke, Jörn-Steffen. 2008. *Mostly harmless econometrics: An empiricist's companion*. Princeton, New Jersey: Princeton University Press.

Archer, Candace C., Biglaiser, Glen., & DeRouen Jr., Karl. 2007. Sovereign bonds and the "democratic advantage": Does regime type affect credit ratings in the developing world? *International Organization*, **61**(2), 341-365.

Arnold, Felix, Kauder, Björn, & Potrafke, Niklas. 2014. Outside earnings, absence, and activity: Evidence from German parliamentarians. *European Journal of Political Economy*, **36**, 147–157.

Ashraf, Nava, Bohnet, Iris and Piankov, Nikita. 2006. "Decomposing trust and trustworthiness." *Experimental Economics* 9(3), 193-208.

Banerjee, Abhijit, Green, Donald, Green, Jennifer, & Pande, Rohini. 2009. *Can voters be primed to choose better legislators? Experimental evidence from rural India*. mimeo.

Banerjee, Abhijit V., Green, Donald, McManus, Jeffrey, & Pande, Rohini. 2014. Are poor voters indifferent to whether elected leaders are criminal or corrupt? *Political Communication*, **31**(3), 391–407.

Banerjee, Abhijit V., & Somanathan, Rohini. 2007. The political economy of public goods: Some evidence from India. *Journal of Development Economics*, **82**, 287–314.

Banks, Arthur S., & Wilson, Kenneth A. 2007, 2012. Cross-national time-series data archive. Databanks International, Jerusalem, Israel; http://www.databanksinternational.com.

Barro, Robert J., & Lee, Jong-Wha. 2005. IMF programs: Who is chosen and what are the effects? *Journal of Monetary Economics*, **52**(7), 1245-1269.

Bartels, Bernhard, & Weder di Mauro, Beatrice. 2013. *A rating agency for Europe – A good idea?* CEPR Discussion Paper No. 9512, London, UK: Centre for Economic Policy Research.

Bar-Isaac, Heski, & Shapiro, Joel. 2011. Credit ratings accuracy and analyst incentives. *American Economic Review: Papers & Proceedings*, **101**(3), 120-130.

Bashir, Omar S., & Lim, Darren J. 2013. Misplaced blame: Foreign aid and the consequences of UN Security Council membership. *Journal of Conflict Resolution*, **57(**3), 509-523.

Bayer, Patrick, Marcoux, Christopher, & Urpelainen, Johannes. 2014. Choosing international organizations: When do states collaborate with the World Bank on environmental projects? *Review of International Organizations*, **9**(4), 413-440.

Bazzi, Samuel, & Clemens, Michael A. 2013. Blunt instruments: Avoiding common pitfalls in identifying the causes of economic growth. *American Economic Journal: Macroeconomics*, **5**(2), 152-186.

Bearce, David H., & Tirone, Daniel C. 2010. Foreign aid effectiveness and the strategic goals of donor governments. *Journal of Politics*, **72**(3), 837-851.

Beaulieu, Emily, Cox, Gary W., & Saiegh, Sebastian. 2012. Sovereign debt and regime type: Reconsidering the democratic advantage. *International Organization*, **66**(4), 709-738.

Beck, Thorsten, Clarke, George, Groff, Alberto, Keefer, Philip, & Walsh, Patrick. 2001. New tools in comparative political economy: The database of political institutions. *World Bank Economic Review*, **15**(1), 165-176.

Becker, Gary S. 1961. *The economics of discrimination* (2nd edition). Chicago, IL: University of Chicago Press.

Bellows, John, & Miguel, Edward. 2008. War and local collective action in Sierra Leone. *Journal of Public Economics*, **93**(11), 1144–1157.

Bermeo, Sarah B. 2011. Foreign aid and regime change: A role for donor intent. *World Development*, **39**(11), 2021-2031.

Berthélemy, Jean-Claude, & Tichit, Ariane. 2004. Bilateral donors' aid allocation decisions: A three-dimensional panel analysis. *International Review of Economics & Finance*, **13**(3), 253-274.

Besley, Timothy. 2004. Joseph Schumpeter lecture: Paying politicians: Theory and evidence, *Journal of the European Economic Association*, **2**(2/3), 193–215.

Besley, Timothy, & Burgess, Robin. 2002. The political economy of government responsiveness: Theory and evidence from India. *Quarterly Journal of Economics*, **117**(4), 1415–1451.

Besley, Timothy, & Case, Anne. 1993. *Does electoral accountability affect economic policy choices? Evidence from gubernatorial term limits*. NBER Working Paper No. 4575.

Besley, Timothy, & Larcinese, Valentino. 2011. Working or shirking? Expenses and attendance in the UK Parliament. *Public Choice*, **146**(3-4), 291–317.

Besley, Timothy, & Persson, Torsten. 2012. The logic of political violence. *Quarterly Journal of Economics*, **126**(3), 1411-1445.

Biglaiser, Glen, Hicks, Brian, & Huggins, Caitlin. 2008. Sovereign bond ratings and the democratic advantage: Portfolio investment in the developing world. *Comparative Political Studies*, **41**(8), 1092-1116.

Biglaiser, Glen, & Staats, Joseph L. 2012. Finding the "democratic advantage" in sovereign bond ratings: The importance of strong courts, property rights protection, and the rule of law. *International Organization*, **66**(3), 515-535.

Binmore, Ken, and Avner Shaked. 2010Experimental economics: Where next?." *Journal of Economic Behavior & Organization*. **73**(1), 87-100.

Bjørnskov, Christian. 2013. *Types of foreign aid*. University of Aarhus Working Paper 2013-08.

Bobba, Matteo, & Powell, Andrew. 2007. *Aid effectiveness: Politics matters*. Inter-American Development Bank, Research Department, Working Paper No. 601.

Block, Steven A., & Vaaler, Paul M. 2004. The Price of democracy: Sovereign risk ratings, bond spreads and political business cycles in developing countries. *Journal of International Money and Finance*, **23**(6), 917-946.

Borensztein, Eduardo, Cowan, Kevin, & Valenzuela, Patricio. 2013. Sovereign ceilings "Lite"? The impact of sovereign ratings on corporate ratings. *Journal of Banking & Finance*, **37**(11), 4014-4024.

Bosworth, Barry P., & Collins, Susan M. 2003. The empirics of growth: An update. *Brookings Papers on Economic Activity*, **2003**(2), 113-206.

Breitwieser, Anja, & Wick, Katharina. 2013. *What we miss by missing data: Aid effectiveness revisited*. University of Vienna, Department of Economics Working Paper No. 1302.

Brennan, Michael J., & Cao, H. Henry. 1997. International portfolio investment flows. *Journal of Finance*, **52**(5), 1851-1880.

Brollo, Fernanda. 2010. *Who is punishing corrupt politicians: Voters or the central government? Evidence from the Brazilian anti-corruption program*. IGIER Working Paper No. 336.

Brooks, Robert, Faff, Robert W., Hillier, David, & Hillier, Joseph. 2004. The national market impact of sovereign rating changes. *Journal of Banking & Finance*, **28**(1), 233-250.

Buchanan, James M., & Tullock, Gordon. 1962. *The calculus of consent*. Ann Arbor: University of Michigan Press.

Bueno de Mesquita, Bruce, & Smith, Alastair. 2013. Aid: Blame it all on "easy money". *Journal of Conflict Resolution*, **57**(3), 524-537.

Burnside, Craig, & Dollar, David. 2000. Aid, policies and growth. *American Economic Review*, **90**(4), 847-868.

Cameron, A. Colin, & Trivedi, Pravin K.. 2005. *Microeconometrics: Methods and applications*. New York: Cambridge University Press.

Cameron, A. Colin, Gelbach, Jonah B., & Miller, Douglas L. 2011. Robust inference with multiway clustering. *Journal of Business and Economic Statistics*, **29**(2), 238–249.

Cantor, Richard Martin, & Packer, Frank. 1996. Determinants and impact of sovereign credit ratings. *Economic Policy Review*, **2**(2), 37-53.

Caplan, Bryan. 2011. *The myth of the rational voter: Why democracies choose bad policies.* Princeton University Press.

Chan, Kalok, Covrig, Vicentiu, & Ng, Lilian. 2005. What determines the domestic bias and foreign bias? Evidence from mutual fund equity allocations worldwide. *Journal of Finance*, **60**(3), 1495-1534.

Chaturvedi, Mayuri, Ghosh, Tilottama, & Bhandari, Laveesh. 2011. Assessing income distribution at the district level for India using nighttime satellite imagery. *Proceedings of the Asia-Pacific Advanced Network*, **32**, 192–217.

Cheibub, José Antonio, Gandhi, Jennifer, & Vreeland, James Raymond. 2010. Democracy and dictatorship revisited. *Public Choice*, **143**(1-2), 67-101.

Chemin, Matthieu. 2012. The welfare effects of criminal politicians: A discontinuity-based approach. *Journal of Law and Economics*, **55**(3), 667–690.

Clemens, Michael A., Radelet, Steven, Bhavnani, Rikhil R., & Bazzi, Samuel. 2012. Counting chickens when they hatch: Timing and the effects of aid on growth. *Economic Journal*, **122**, 590-617.

Christensen, Zachary, Homer, Dustin, & Nielson, Daniel L. 2011. Dodging adverse selection: How donor type and governance condition aid's effects on school enrollment. *World Development*, **39**(11), 2044-2053.

Cordes, Till. 2012. *Where is the democratic advantage in the sovereign debt market? It is veto players, not contested elections*. Working Paper, Berlin, Germany: Hertie School of Governance.

Creasey, Ellyn, Rahman, Ahmed S., & Smith, Katherine A. 2015. Does nation building spur economic growth? *Economic Inquiry*, **53**(1), 660-680.

De Haan, Jakob, & Amtenbrink, Fabian. 2011. *Credit rating agencies*. DNB Working Paper No. 278, Amsterdam, Netherlands: De Nederlandsche Bank.

DiGiuseppe, Matthew R., Barry, Colin M., & Frank, Richard W. 2012. Good for the money: International finance, state capacity, and internal armed conflict. *Journal of Peace Research*, **49**(3), 391-405.

Dreher, Axel, & Gassebner, Martin. 2008. Does political proximity to the U.S. cause terror? *Economics Letters*, **99**(1), 27-29.

Dreher, Axel., Eichenauer, Vera., & Gehring, Kai. (2014). *Geopolitics, Aid and Growth* (No. 0575). University of Heidelberg, Department of Economics.

Dreher, Axel, Marchesi, Silvia, & Vreeland, James. 2008. The political economy of IMF forecasts. *Public Choice*, **137**(1-2), 145-171.

Doucouliagos, Hristos, & Paldam, Martin. 2009. The aid effectiveness literature: The sad results of 40 years of research. *Journal of Economic Surveys*, **23(**3), 433-461.

Dreher, Axel, Gould, Matthew, Rablen, Matthew, & Vreeland, James Raymond. 2014. The determinants of election to the United Nations Security Council. *Public Choice*, **158**, 51-83.

Dreher, Axel, Klasen, Stephan, Vreeland, James Raymond, & Werker, Eric. 2013. The costs of favoritism: Is politically-driven aid less effective? *Economic Development and Cultural Change*, **62,** 157-191.

Dreher, Axel, Sturm, Jan-Egbert, & Vreeland, James Raymond. 2009a. Development aid and international politics: Does membership on the UN Security Council influence World Bank decisions? *Journal of Development Economics*, **88**(1), 1-18.

Dreher, Axel, Sturm, Jan-Egbert, & Vreeland, James Raymond. 2009b. Global horse trading: IMF loans for votes in the United Nations Security Council. *European Economic Review*, **53**, 742-757.

Drometer, Marcus. 2013. *Institutional quality and foreign aid*. Ifo, Munich.

Dunning, Thad. 2004. Conditioning the effects of aid: Cold War politics, donor credibility, and democracy in Africa. *International Organization*, **58**, 409-423.

Durbarry, Ramesh, Gemmell, Norman, & Greenaway, David. 1998. *New evidence on the impact of foreign aid on economic growth*. CREDIT Research Paper 98/8.

Durbin, Erik, & Ng, David. 2005. The sovereign ceiling and emerging market corporate bond spreads. *Journal of International Money and Finance*, **24**(4), 631-649.

Dutta, Bhaskar, & Gupta, Poonam. 2012 (April). *How do Indian voters respond to candidates with criminal charges: Evidence from the 2009 Lok Sabha elections*. MPRA Paper No. 38417.

Easterly, William, & Levine, Ross. 1997. Africa's growth tragedy: Policies and ethnic division. *Quarterly Journal of Economics*, **112**(4), 1203-1250.

Faye, Michael, & Niehaus, Paul. 2012. Political aid cycles. *American Economic Review*, **102**(7), 3516-3530.

Fehr, Ernst, & Fischbacher, Urs. 2003. The nature of human altruism. *Nature* **425**(6960), 785-791.

Ferraz, Claudio, & Finan, Frederico. 2011. Electoral accountability and corruption: Evidence from the audits of local governments. *American Economic Review*, **101**(4), 1274–1311.

Ferraz, Claudio & Finan, Frederico. 2008. Exposing corrupt politicians: The effects of Brazil's publicly released audits on electoral outcomes. *Quarterly Journal of Economics*, **123**, 703-745.

Ferrer-i-Carbonell, Ada, & Frijters, Paul. 2004. How important is methodology for the estimates of the determinants of happiness? *Economic Journal*, **114**(497), 641-659.

Ferri, Giovanni, Liu, Li-gang, & Stiglitz, Joseph E. (1999). The procyclical role of rating agencies: Evidence from the East Asian crisis. *Economic Notes*, **28**(3), 335-355.

Ferri, Giovanni, Lacitignola, Punziana, & Lee, Jeong Yeon. 2013. Foreign ownership and the credibility of national rating agencies: Evidence from Korea. *Journal of Comparative Economics*, **41**(3), 762-776.

Fisman, Raymond, Schulz, Florian, & Vig, Vikrant. 2014. The private returns to public office. *Journal of Political Economy*, **122**(4), 806–862.

Fleck, Robert K., & Kilby, Christopher. 2006. How do political changes influence U.S. bilateral aid allocations? Evidence from panel data. *Review of Development Economics*, **10**(2), 210-223.

Fratzscher, Marcel, & Reynaud, Julien. 2011. IMF Surveillance and financial markets—A political economy analysis. *European Journal of Political Economy*, **27**(3), 405-422.

French, Kenneth R., & Poterba, James M. 1991. Investor diversification and international equity markets. *American Economic Review*, **81**(2), 222-226.

Frey, Bruno S., and Alois Stutzer. 2010. Happiness and public choice. *Public Choice* 144(3-4), 557-573.

Fuchs, Andreas, & Gehring, Kai. 2013. *The home bias in sovereign ratings*. University of Heidelberg Department of Economics Discussion Paper Series 552.

Gagliarducci, Stefano, Nannicini, Tommaso, & Naticchioni, Paolo. 2010. Moonlighting politicians. *Journal of Public Economics*, **94**(9), 688–699.

Gagliarducci, Stefano, Nannicini, Tommaso, & Naticchioni, Paolo. 2011. Electoral rules and politicians' behavior: A micro test. *American Economic Journal: Economic Policy*, **3**(3), 144–174.

Gaillard, Norbert. 2012. *A Century of Sovereign Ratings*, New York, NY: Springer.

Galiani, Sebastian, Knack, Stephen, Xu, Lixin C., & Zou, Ben. 2014. *The effect of aid on growth: Evidence from a quasi-experiment*. World Bank Policy Research Working Paper No. 6865.

Gary, Aurore, & Maurel, Mathilde. 2013. *The effect of donors' policy coherence on growth*. University of Paris I.

Gehrig, Thomas. 1993. An information based explanation of the domestic bias in international equity investment. *Scandinavian Journal of Economics*, **95**(1), 97-109.

Gehring, Kai. 2013. Who benefits from economic freedom? Unraveling the effect of economic freedom on subjective well-being. *World Development*, **50**, 74-90.

Gehring, Kai, Kauffeldt, T. Florian, & Vadlamannati, Krishna. 2015. *Crime, Incentives and Political Effort: A Model and Empirical Application for India*. Courant Research Centre: Poverty, Equity and Growth - Discussion Papers 170.

Giannetti, Mariassunta, & Yafeh, Yishay. 2012. Do cultural differences between contracting parties matter? Evidence from syndicated bank loans. *Management Science*, **58**(2), 365-383.

Grinblatt, Mark, & Keloharju, Matti. 2001. How distance, language, and culture influence stockholdings and trades. *Journal of Finance*, **56**(3), 1053-1073.

Guiso, Luigi, Sapienza, Paola, & Zingales, Luigi. 2006. Does culture affect economic outcomes? *Journal of Economic Perspectives*, **20**(2), 23-48.

Guiso, Luigi, Sapienza, Paola, & Zingales, Luigi. 2009. Cultural biases in economic exchange? *Quarterly Journal of Economics*, **124**(3), 1095-1131.

Güttler, André, & Wahrenburg, Mark. 2007. The adjustment of credit ratings in advance of defaults. *Journal of Banking & Finance*, **31**(3), 751-767.

Haque, Nadeem U., Mark, Nelson, & Mathieson, Donald J. 1998. *The relative importance of political and economic variables in creditworthiness ratings*. IMF Working Paper WP/98/46, Washington, DC: International Monetary Fund.

Headey, Derek. 2008. Geopolitics and the effect of foreign aid on economic growth: 1970-2001. *Journal of International Development*, **20**, 161-180.

Henderson, Vernon, Storeygard, Adam, & Weil, David N. 2011. A bright idea for measuring economic growth. *American Economic Review*, **101**(3), 194–199.

Hill, Paula, Brooks, Robert, & Faff, Robert. 2010. Variations in sovereign credit quality assessments across rating agencies. *Journal of Banking & Finance*, **34**(6), 1327-1343.

Hosli, Madeleine O., Moody, Rebecca, O'Donovan, Bryan, Kaniovski, Serguei, & Little, Anna C. H. 2011. Squaring the circle? Collective and distributive effects of United Nations Security Council reform. *Review of International Organizations*, **6**(2), 163-87.

International Monetary Fund. 2005. International Financial Statistics. Washington, D.C. Kahneman, Daniel. 2011. *Thinking, Fast and Slow*. Macmillan.

Karlan, Dean S. 2005. Using experimental economics to measure social capital and predict financial decisions. *American Economic Review* **95**(5): 1688-1699.

Kaminsky, Graciela, & Schmukler, Sergio L. 2002. Emerging market instability: Do sovereign ratings affect country risk and stock returns? *World Bank Economic Review*, **16**(2), 171-195.

Kapur, Devesh, & Vaishnav, Milan. 2011 (December). *Quid pro quo: Builders, politicians, and election finance in India*. Center for Global Development Working Paper No. 276.

Keefer, Philip, & Khemani, Stuti. 2009. When do legislators pass on pork? The role of political parties in determining legislator effort. *American Political Science Review*, **103**(01), 99–112.

Kilby, Christopher. 2009. The political economy of conditionality: An empirical analysis of World Bank loan disbursements. *Journal of Development Economics*, **89(**1), 51-61.

Kilby, Christopher. 2011. *Assessing the contribution of donor agencies to aid effectiveness: The impact of World Bank preparation on project outcomes*. mimeo.

Kilby, Christopher. 2013. The political economy of project preparation: an empirical analysis of World Bank projects. *Journal of Development Economics*, **105**, 211-225.

Kilby, Christopher, & Dreher, Axel. 2010. The impact of aid on growth revisited: Do donor and recipient characteristics make a difference? *Economics Letters*, **107(**3), 338-340.

Kilka, Michael, & Weber, Martin. 2000. Home bias in international stock return expectations. *Journal of Psychology and Financial Markets*, **1**(3-4), 176-192.

Kolo, Philipp. 2012. *Measuring a new aspect of ethnicity - The appropriate diversity index*. Ibero America Institute for Economic Research (IAI) Discussion Papers No. 221. Göttingen, Germany: Ibero-America Institute for Economic Research.

Kumar, Rohit. 2010 (February). *Vital stats: Private members bills in Lok Sabha*. PRS Legislative Research.

Kuziemko, Ilyana, & Werker, Eric. 2006. How much is a seat on the Security Council Worth? Foreign aid and bribery at the United Nations. *Journal of Political Economy*, **114**(5), 905-930.

Laeven, Luc, & Valencia, Fabián. 2012. Systemic banking crises database: An update. IMF Working Paper No. 12/163. Washington, DC: International Monetary Fund.

Lewis, M. Paul. 2009. *Ethnologue: Languages of the world*. Sixteenth edition, Dallas, Texas: SIL International.

Ling, Ding. 2012. Dagong ou les contradictions d'une agence de notation à la fois internationale et patriotique. *Outre-terre*, **32**(2), 67-72.

MacKinnon, James G., & Webb, Matthew D. 2015 (February). *Wild bootstrap inference for wildly different cluster sizes*. Queen's Economics Department Working Paper No. 1314.

Maddala, Gangadharrao S. 1983. *Limited-dependent and qualitative variables in econometrics*. Cambridge: Cambridge University Press.

Marshall, Monty G., & Jaggers, Keith. 2003. *Polity IV project country reports*. CIDUM, University of Maryland.

Marshall, Monty G., Gurr, Ted Robert, & Jaggers, Keith. 2013. *Polity IV project: Political regime characteristics and transitions, 1800-2012*, Vienna, VA: Center for Systemic Peace.

Mayer, Thierry, & Zignago, Soledad. 2011. *Notes on CEPII's Distances Measures (GeoDist)*, CEPII Working Paper No. 2011-25, Paris, France: Centre d'Etudes Prospectives et d'Informations Internationales.

McCallum, John T. 1995. National borders matter: Canada-U.S. regional trade patterns, *American Economic Review*, **85**(3), 615-623.

McCrary, Justin. 2008. Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics*, **142**(2), 698–714.

Melitz, Jacques. 2008. Language and foreign trade. *European Economic Review*, **52**(4), 667-699.

Midtgaard, Trude M., Vadlamannati, Krishna Chaitanya, & Indra de Soysa. 2014. Does the IMF cause civil war? A comment. *Review of International Organizations*, **9**, 107-124.

Minoiu, Camelia, & Reddy, Sanjay G. 2010. Development aid and economic growth. *Quarterly Review of Economics and Finance*, **50**(1), 27-39.

Mishkin, Frederic S. 2011. Over the cliff: From the subprime to the global financial crisis. *Journal of Economic Perspectives*, **25**(1), 49-70.

Mocan, Naci, & Altindag, Duha T. 2013. Salaries and work effort: An analysis of the European Union parliamentarians. *The Economic Journal*, **123**(573), 1130–1167.

Morse, Adair, & Shive, Sophie. 2011. Patriotism in your portfolio. *Journal of Financial Markets*, **14**(2), 411-440.

Nickell, Pamela, Perraudin, William, & Varotto, Simone. 2000. Stability of rating transitions, *Journal of Banking & Finance*, **24**(1-2), 203-227.

Nizalova, Olena, & Murtazashvili, Irina. 2012. *Exogenous treatment and endogenous factors: Vanishing of omitted variable bias on the interaction term*. IZA DP No. 6282.

Nooruddin, Irfan, & Chhibber, Pradeep. 2008. Unstable politics: Fiscal space and electoral volatility in the Indian states. *Comparative Political Studies*, **41**(8), 1069–91.

Nooruddin, Irfan, & Vreeland, James Raymond. 2010. The Effect of IMF programs on public wages and salaries. In: Wilkinson, Rorden, & Clapp, Jennifer (eds.), *Global Governance, Poverty, and Inequality*, London: Routledge.

Nunn, Nathan, & Qian, Nancy. 2013. U.S. food aid and civil conflict. *American Economic Review*, **103**(3), 86-92.

OECD. 2003. Harmonising donor practices for effective aid delivery. *DAC Guidelines and Reference Series*, Paris: OECD.

Odedokun, Matthew. 2003. *Analysis of deviations and delays in aid disbursements*. UNU WIDER Discussion Paper No. 2003/26.

Paul, Samuel, & Vivekananda, M. 2004. Holding a mirror to the new Lok Sabha. *Economic and Political Weekly*, **39**(45), 4927–4934.

Prakash, Nishith, Rockmore, Marc, & Uppal, Yogesh. 2014. *Do criminal representatives hinder or improve constituency outcomes? Evidence from India*. Working Paper No. 2014-20, University of Connecticut, Department of Economics.

Presbitero, Andrea F., Udell, Gregory F., & Zazzaro, Alberto. 2014. The home bias and the credit crunch: A regional perspective. *Journal of Money, Credit and Banking*, **46**(s1), 53-85.

Rajan, Raghuram G., & Subramanian, Arvind. 2008. Aid and growth. *Review of Economics and Statistics*m, **90**(4), 643-665.

Ravina, Enrichetta. 2008. *Love & loans: The effect of beauty and personal characteristics in credit markets*. American Law & Economics Association Annual Meetings Paper 67, Berkeley, CA: The Berkeley Electronic Press.

Reinhart, Carmen M. 2002. Default, currency crises, and sovereign credit ratings. *World Bank Economic Review*, **16**(2), 151-170.

Reinhart, Carmen M., & Rogoff, Kenneth. 2009. *This time is different: Eight centuries of financial folly*. Princeton, NJ: Princeton University Press.

Roeder, Philip G. 2001. Ethnolinguistic fractionalization (ELF) indices, 1961 and 1985. http://weber.ucsd.edu/proeder/elf.htm.

Roodman, David. 2007. The anarchy of numbers: Aid, development, and cross-country empirics, *World Bank Economic Review*, **21**(2), 255-277.

Sachs, Jeffrey D., & Warner, Andrew. 1995. Economic reform and the process of global integration. *Brookings Papers on Economic Activity*, **1995**(1), 1-95.

Sarkissian, Sergei, & Schill, Michael J. 2004. The overseas listing decision: New evidence of proximity preference. *Review of Financial Studies*, **17**(3), 769-809.

Shleifer, Andrei. 2012. Psychologists at the Gate: A Review of Daniel Kahneman's" Thinking, Fast and Slow". *Journal of Economic Literature*, 1080-1091.

Shiller, Robert J., Kon-Ya, Fumiko, & Tsutsui, Yoshiro. 1996. Why did the Nikkei crash? Expanding the scope of expectations data collection. *Review of Economics and Statistics*, **78**(1), 156-164.

Shin, Yoon S., & Moore, William T. 2003. Explaining credit rating differences between Japanese and U.S. agencies. *Review of Financial Economics*, **12**(4), 327-344.

Snyder, James M., & Strömberg, David. 2010. Press coverage and political accountability, *Journal of Political Economy*, **118**(2), 355–408.

Spolaore, Enrico, & Wacziarg, Romain. 2009. The diffusion of development. *Quarterly Journal of Economics*, **124**(2), 469-529.

Stiglitz, Joseph E. 1973. Approaches to the economics of discrimination. *American Economic Review*, **63**(2), 287-295.

Stone, Randall. 2004. The political economy of IMF lending in Africa. *American Political Science Review*, **98**(4), 577-92.

Stone, Randall. 2008. The scope of IMF conditionality. *International Organization*, **62**(4), 589-620.

Strezhnev, Anton, & Voeten, Erik. 2012. *United Nations General Assembly voting data*. Available at http://hdl.handle.net/1902.1/12379 (accessed 25 October 2012).

Strong, Norman, & Xu, Xinzhong. 2003. Understanding the equity home bias: Evidence from survey data. *Review of Economics and Statistics*, **85**(2), 307-312.

Strömberg, David. 2001. Mass media and public policy. *European Economic Review*, **45**, 652–663.

Strömberg, David. 2004. Mass media competition, political competition, and public policy. *The Review of Economic Studies*. **71**, 265–284.

Svaleryd, Helena, & Vlachos, Jonas. 2009. Political rents in a non-corrupt democracy. *Journal of Public Economics*, **93**, 355–372.

S&P. 2012. *How we rate sovereigns*. Standard & Poor's global credit portal: Ratings direct. New York, NY: Standard & Poor's, Available at http://www.standardandpoors.com/spf/ratings/How_We_Rate_Sovereigns_3_13_12.pdf (accessed 21 March 2013).

Tesar, Linda L., & Werner, Ingrid M. 1995. Home bias and high turnover. *Journal of International Money and Finance*, **14**(4), 467-492.

Tomz, Michael, & Wright, Mark L.J. 2007. Do countries default in "bad times"? *Journal of the European Economic Association*, **5**(2-3), 352-360.

Tullock, Gordon. 1980. Efficient rent-seeking. *Pages 97–112 of:* Buchanan, James M., Tollison, Robert D., & Tullock, Gordon (eds.), *Toward a theory of the rent-seeking society*. College Station: Texas A&M University Press.

Tversky, Amos, & Kahneman, Daniel. 1974. Judgment under uncertainty: Heuristics and biases. *Science*, 185(4157), 1124-1131.

Vaaler, Paul M., Schrage, Burkhard N., & Block, Steven A. 2006. Elections, opportunism, partisanship and sovereign ratings in developing countries. *Review of Development Economics*, **10**(1), 154-170.

Vaishnav, Milan. 2011. *The market for criminality: Money, muscle and elections in India*. mimeo.

Vaishnav, Milan. 2012. *The merits of money and "muscle": Essays on criminality, elections and democracy in India*. mimeo.

Van Nieuwerburgh, Stijn, & Veldkamp, Laura. 2009. Information immobility and the home bias puzzle. *Journal of Finance*, **64**(3), 1187-1215.

Voeten, Eric. 2000. Clashes in the assembly. *International Organization*, **54**(2), 185–215.

Vreeland, James Raymond, & Dreher, Axel. 2014. *The political economy of the United Nations Security Council – Money and influence*. Cambridge University Press.

Wacziarg, Romain T., & Welch, Karen Horn. 2008. Trade liberalization and growth: New evidence. *World Bank Economic Review*, **22**(2), 187-231.

Werker, Eric D. 2012. The political economy of bilateral foreign aid. In: Gerard Caprio (ed.), *Handbook of Safeguarding Global Financial Stability: Political, Social, Cultural, and Economic Theories and Models*. U.K., Academic Press: 47-58.

Werker, Eric D., Ahmed, Faisal Z., & Cohen, Charles. 2009. How is foreign aid spent? Evidence from a natural experiment. *American Economic Journal: Macroeconomics*, **1**(2), 225-244.

White, Lawrence J. 2010. Markets: The credit rating agencies. *Journal of Economic Perspectives*, **24**(2), 211-226.

Wolf, Holger C. 2000. Intranational home bias in trade. *Review of Economics and Statistics*, **82**(4), 555-56.

World Bank. 2005, 2007, 2012. World Development Indicators. Washington, D.C.

Yu, Shu, De Haan, Jakob, & Beugelsdijk, Sjoerd. 2015. Trade, trust and the rule of law. *European Journal of Political Economy*, **37**, 102-115.

Zheng, Liping. 2012. Are sovereign credit ratings objective? A tale of two agencies. *Journal of Applied Finance & Banking*, **2**(5), 43-61.

V Appendices

Appendix I.A: Definitions and sources

Variable	Definition	Original Source
UNSC Membership	Share of years a country has served as a temporary member on the UNSC in a given period.	Dreher et al. (2009b)
US Bilateral Development Aid	Official Development Aid Disbursements from the US in % of GDP.	DAC (2012), Table DAC2a ODA Disbursements, February 2012
Democracy	Dummy that is 1 if the country is a Democracy during at least half the period under consideration.	Cheibub et al. (2010)
Dummy for Africa	Dummy that is 1 if the recipient is an African country.	World Bank (2012)
Burnside and Dollar specification (4-year periods)		
GDP p.c. growth	Average over annual growth rates of real GDP p.c. based on constant local currency.	World Bank (2007)*
Net ODA	ODA (OA) total net in % of GDP.	DAC (2007), Table DAC2a*
Region Dummies	Dummies for Sub-Saharan Africa and East Asia.	Clemens et al. (2012)
Log Initial GDP/capita	Logarithm of initial GDP p.c. in International prices.	Penn World Tables 6.2*
Budget Balance	Overall Budget Balance, including grants. Measured as cash surplus/deficit in % of GDP.	World Bank (2005, 2007), IMF (2005)*
Inflation	Natural log of (1+ Consumer Price Inflation).	World Bank (2005, 2007), IMF (2005)*
M2 (% of GDP)	Money and Quasi-Money (M2) in % of GDP.	World Bank (2007)*
Institutional Quality	First non-missing value of the ICRG composite index [0, 10].	ICRG*
Assassinations	Average number of Assassinations in a given phase.	Banks (2012, 2007)*
Ethnolinguistic Fractionalization	Ethnolinguistic Fractionalization in a country in a given period.	Easterly and Levine (1997), Roeder (2001)*
Assassinations x Ethnolinguistic Fractionalization	Interaction between Assassinations and Ethnolinguistic Fractionalization.	Banks (2012, 2007), Easterly and Levine (1997), Roeder (2001)*
Policy	Good Policy Index based on Budget Balance/GDP, Inflation and Trade Openness (cf. Burnside and Dollar 2000).	Clemens et al. (2012)
Openness	Wacziarg-Welch (2008) extension of the initial Sachs and Warner (1995) Openness Index.	Wacziarg and Welch (2008), updated by Clemens et al. (2012)*

Rajan and Subramanian specification (5-year periods)

GDP p.c. Growth	Average annual growth rate of real GDP p.c. in constant International Dollars.	Penn World Tables 6.2 and World Bank (2007) for the year 2005*
Net ODA	ODA total net in % of GDP.	DAC (2007), Table DAC2a*
Log Initial GDP/capita	Logarithm of initial GDP p.c. in International Prices.	Penn World Tables 6.2*
Institutional Quality	Period averages of the sum of three components (Bureaucratic Quality, Rule of Law and Corruption) of the ICRG index, normalized to one.	ICRG*
Geography	Combination of the average number of frost days per month in winter and the fraction of a country's area in the tropics.	Bosworth and Collins (2003)*
Revolutions	Average number of Revolutions in a period.	Banks (2007)*
Initial Life Expectancy	Natural logarithm of first non-missing value in each period of Total Life Expectancy.	World Bank (2007)*
Inflation	Natural log of (1+consumer price inflation).	World Bank (2005, 2007), IMF (2005)*
Budget Balance	Overall Budget Balance, including grants. Measured as cash surplus/deficit as % of GDP.	World Bank (2005, 2007), IMF (2005)*
Ethnolinguistic Fractionalization	Ethnolinguistic Fractionalization in a country in a given period.	Easterly and Levine (1997), Roeder (2001)*
Initial Policy	First non-missing value of the Wacziarg-Welch openness dummy.	Wacziarg and Welsh (2008)*
M2 (% of GDP)	Money and quasi-money (M2) in % of GDP.	World Bank (2007)*

Bueno de Mesquita and Smith specification

Democracy	POLITY IV Democracy Index, in the last year of the previous period, transformed to a [0,1] scale.	Marshall and Jaggers (2003)**
Democracy x UNSC Membership (t-2)	Interaction between Democracy Index and the share of years the country was on the UNSC in the respective period.	Dreher et al. (2009b), Marshall and Jaggers (2003)**
Population	Logarithm of Population Size.	World Bank (2007)**
Log Initial GDP	Logarithm of Initial GDP p.c. (in constant 2000 US$).	World Bank (2007)**
Aid	Total Aid (bilateral and multilateral) in % of GDP.	World Bank (2007)**
GDP p.c. growth	GDP p.c. growth rate over a four-year-period in constant 2000 US$.	World Bank (2007)**

Notes: DAC is the OECD's Development Assistance Committee; ICRG is the International Country Risk Guide.

* Our source is Clemens et al. (2012), http://www.cgdev.org/doc/Working%20Papers/CRBB-Replication-Files.zip, accessed 06.06.2012.

More details are provided in "Technical Appendix to Counting chickens when they hatch: Timing and the effects of aid on growth," http://www.cgdev.org/doc/Working%20Papers/counting_chickens_technical_appendix.pdf, accessed 06.06.2012.

** Our source is Bueno de Mesquita and Smith (2010), http://politics.as.nyu.edu/staging/IO/5347/PerniciousEffectUNSC.zip, accessed 08.12.2012.

Appendix I.B: Descriptive Statistics

Variable	Count	Mean	Standard deviation	Min.	Max.
Burnside and Dollar specification (4-year-periods)					
GDP p.c. growth	361	1.21	3.35	-12.96	17.05
Net ODA (% of GDP)	361	4.54	6.27	-0.13	42.52
Log Initial GDP/capita	361	8.03	0.78	6.14	9.96
Budget Balance	361	-0.08	0.65	-7.25	4.71
Inflation	229	0.28	0.45	-0.01	3.22
M2 (% of GDP)	361	0.28	0.14	0.02	1.02
Institutional Quality	361	4.35	1.49	1.58	8.14
Assassinations	361	0.49	1.35	0	11.50
Ethnolinguistic Fractionalization	361	0.46	0.30	0	0.93
Policy	361	1.45	1.41	-5.48	3.50
Openness	229	0.29	0.43	0	1
Rajan and Subramanian specification (5-year-periods)					
GDP p.c. growth	351	1.48	3.06	-12.30	9.36
Net ODA (% of GDP)	351	4.28	6.05	-0.06	40.27
Log Initial GDP/capita	351	8.16	0.85	5.85	10.27
Institutional Quality	351	4.57	1.68	1.58	9.50
Geography	351	-0.50	0.77	-1.04	1.53
Revolutions	351	0.26	0.42	0	2.60
Initial Life Expectancy	351	61.92	10.04	36.55	79.41
Inflation	351	0.23	0.49	0	4.19
Budget Balance	351	-0.09	0.52	-5.51	2.35
Ethnolinguistic Fractionalization	351	0.44	0.30	0	0.90
Initial Policy	351	0.45	0.50	0	1
M2 (% of GDP)	351	3.01	7.64	0	49.85
Bueno de Mesquita and Smith specification (4-year moving averages)					
Democracy	3378	0.44	0.35	0	1
Population	3378	15.82	1.53	12.27	20.96
Log Initial GDP	3378	6.69	1.08	4.49	9.71
Total Aid (from all sources, % GDP)	3378	6.25	8.29	0	68.30
GDP p.c. growth	3378	6.89	17.81	-80.73	246.22

Appendix I.C: Full regression specifications

Table C.1: Burnside and Dollar & Rajan and Subramanian

	Burnside and Dollar		Rajan and Subramanian	
	(1)		(2)	
Aid (t-1)	0.453**	(0.189)	0.356**	(0.148)
Aid (t-1) squared	-0.010**	(0.004)	-0.007	(0.004)
UNSC (t-2)	-1.649*	(0.992)	-0.947	(1.402)
UNSC (t-2)*Aid (t-1)	-1.222***	(0.369)	-1.365**	(0.647)
GDP p.c. growth	-4.267*	(2.318)	-9.920***	(1.432)
Assassinations	-0.255	(0.230)		
Assassinations * Ethnolinguistic Fractionalization	0.439	(0.449)		
M2/GDP	0.801	(3.817)		
Policy	0.858***	(0.199)		
Initial Life Expectancy			-0.009	(0.079)
Initial Policy			0.675	(0.459)
Inflation			-1.486***	(0.368)
M2/GDP			-0.023	(0.034)
Budget Balance			0.131	(0.147)
Revolutions			-0.767**	(0.363)
First difference?	Yes		Yes	
Adj. R-Squared	0.29		0.31	
Number of Observations	361		351	
Number of Countries	54		64	

Notes: Dependent variable is growth of real GDP per capita. Full regression results corresponding to Table 1, columns 7 and 8. Note that time-invariant variables are dropped in the regressions using first differences. All "Burnside and Dollar" regressions include Initial GDP/capita, Ethnic Fractionalization, Assassinations, Ethnic Fractionalization*Assassinations, dummies for Sub-Saharan Africa and East Asia, Institutional Quality, M2/GDP (lagged), Policy, and period dummies. The dependent variable covers the period 1970-2005 (corresponds to Clemens et al. (2012) Table 7, columns 1 and 7). All "Rajan and Subramanian" regressions include Initial GDP/capita, Initial Policy, (log) Initial Life Expectancy, Geography, Institutional Quality, (log) Inflation, Initial M2/GDP, Budget Balance/GDP, Revolutions, Ethnic Fractionalization, and dummies for Sub-Saharan Africa and East Asia. The dependent variable covers the period 1966-2005 (using the full extended sample provided by Clemens et al. (2012), compare their Table 9). Standard errors in parentheses (clustered at the recipient country level). * p<0.10, ** p<0.05, *** p<0.01.

Table I.C.2: Bueno de Mesquita and Smith

	(1)		(2)	
	Coef.	Std. err.	Coef.	Std. err.
UNSC (t-2)	0.273***	(0.103)	2.774*	(1.568)
Aid (t-1)	-0.93	(1.299)	0.247	(0.170)
UNSC (t-2)*Aid (t-1)	-0.024	(0.077)	-0.175***	(0.060)
Democracy	-4.634**	(2.221)	-1.671	(2.203)
Democracy* UNSC Membership (t-2)	0.343	(1.871)	-6.063	(4.494)
Population	-41.526***	(12.218)	-13.386	(15.991)
Log Initial GDP	-23.804***	(4.735)	-16.265***	(5.373)
Sample	all		Africa	
Country Fixed Effects	Yes		Yes	
Region Fixed Effects	No		No	
Year Dummies	Yes		Yes	
Regional Trend Variables	Yes		Yes	
Adj. R-Squared	0.45		0.41	
Number of Observations	3378		1272	
Number of Countries	119		42	

Notes: The dependent variable is growth of real GDP per capita. Full regression results corresponding to Table 3, columns 7 and 8. All regressions include (log) Population Size, (log) GDP per capita, the level of Democracy and its interaction with UNSC Membership. The sample is based on the data made available by BdM/Smith. Standard errors in parentheses (clustered at the recipient country level). * p<0.10, ** p<0.05, *** p<0.01

Appendix I.D: Marginal effect of a change in aid

$$[Y_{i,t} - Y_{i,t-1}] = \beta_0 + \beta_1 * [Aid_{i,t-1} - Aid_{i,t-2}] + \beta_2 * [Aid_{i,t-1}{}^2 - Aid_{i,t-2}{}^2] + \beta_3 * UNSC_{i,t-2} + \beta_3 * UNSC_{i,t-2} + \beta_4 * [Aid_{i,t-1} - Aid_{i,t-2}] * UNSC_{i,t-2} + \beta_5 * [Controls_{i,t} - Controls_{i,t-1}] + \varepsilon_{i,t}$$

$$[Y_{i,t} - Y_{i,t-1}] = \beta_0 + \beta_1 * [Aid_{i,t-1} - Aid_{i,t-2}] + \beta_2 * [Aid_{i,t-1} + Aid_{i,t-2}] * [Aid_{i,t-1} - Aid_{i,t-2}] + \beta_3 * UNSC_{i,t-2} + \beta_4 * [Aid_{i,t-1} - Aid_{i,t-2}] * UNSC_{i,t-2} + \beta_5 * [Controls_{i,t} - Controls_{i,t-1}] + \varepsilon_{i,t}$$

$$[Y_{i,t} - Y_{i,t-1}] = \beta_0 + \beta_1 * [Aid_{i,t-1} - Aid_{i,t-2}] + \beta_2 * [Aid_{i,t-1} - Aid_{i,t-2} + Aid_{i,t-2} + Aid_{i,t-2}] * [Aid_{i,t-1} - Aid_{i,t-2}] + \beta_3 * UNSC_{i,t-2} + \beta_4 * [Aid_{i,t-1} - Aid_{i,t-2}] * UNSC_{i,t-2} + \beta_5 * [Controls_{i,t} - Controls_{i,t-1}] + \varepsilon_{i,t}$$

Replacing $Aid_{i,t-1} - Aid_{i,t-2} = \Delta Aid_{i,t-1}$:

$$[Y_{i,t} - Y_{i,t-1}] = \beta_0 + \beta_1 * [\Delta Aid_{i,t-1}] + \beta_2 * [\Delta Aid_{i,t-1} + Aid_{i,t-2} + Aid_{i,t-2}] * [\Delta Aid_{i,t-1}] + \beta_3 * UNSC_{i,t-2} + \beta_4 * [\Delta Aid_{i,t-1})] * UNSC_{i,t-2} + \beta_5 * [Controls_{i,t} - Controls_{i,t-1}] + \varepsilon_{i,t}$$

$$\frac{\delta[Y_{i,t} - Y_{i,t-1}y]}{\delta \Delta Aid_{i,t-1}} = \beta_1 + \beta_2 * 2 * Aid_{i,t-2} + \beta_2 * 2 * \Delta Aid_{i,t-1} + \beta_4 * UNSC_{i,t-2}$$

Appendix II to "The Home Bias in Sovereign Ratings"

May 2015

Contents:

Agency-specific Regressions

Appendix II, A17: Determinants of sovereign ratings (only control variables, by agency)

Appendix II, A18: Adj. R-squared and number of observations of OLS models in Table 6

Exploration of Transmission Channels

Appendix II, A19: Marginal effect of cultural distance language at different levels of bilateral trust (Feri only)

Further Robustness Checks

Appendix II, A20: Home bias in sovereign ratings (all agencies pooled, Heckman selection model)

Appendix II, A21: Unsolicited ratings and home bias in sovereign ratings (S&P only)

Additional References

Appendix II, A22: Detailed sources of overview tables

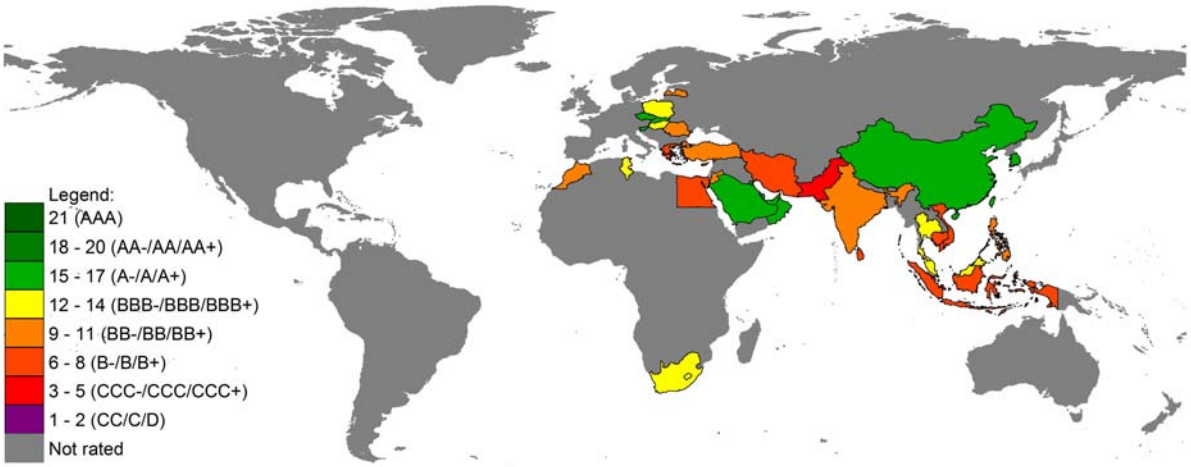
Appendix II, A0: Translation of sovereign ratings into numerical values (21-point scale)

CI CYP (KWT)	**Dagong** CHN	**DBRS** CAN	**Feri** DEU	**Fitch** USA (FRA)	**Moody's** USA	**JCR** JPN	**R&I** JPN	**S&P** USA	Numerical scale
AAA	AAA	AAA	AAA	AAA	Aaa	AAA	AAA	AAA	21
AA+	AA+	AAH	AA+	AA+	Aa1	AA+	AA+	AA+	20
AA	AA	AA	AA	AA	Aa2	AA	AA	AA	19
AA-	AA-	AAL	AA-	AA-	Aa3	AA-	AA-	AA-	18
A+	A+	AH	A+	A+	A1	A+	A+	A+	17
A	A	A	A	A	A2	A	A	A	16
A-	A-	AL	A-	A-	A3	A-	A-	A-	15
BBB+	BBB+	BBBH	BBB+	BBB+	Baa1	BBB+	BBB+	BBB+	14
BBB	BBB	BBB	BBB	BBB	Baa2	BBB	BBB	BBB	13
BBB-	BBB-	BBBL	BBB-	BBB-	Baa3	BBB-	BBB-	BBB-	12
BB+	BB+	BBH	BB+	BB+	Ba1	BB+	BB+	BB+	11
BB	BB	BB	BB	BB	Ba2	BB	BB	BB	10
BB-	BB-	BBL	BB-	BB-	Ba3	BB-	BB-	BB-	9
B+	B+	BH	B+	B+	B1	B+	B+	B+	8
B	B	B	B	B	B2	B	B	B	7
B-	B-	BL	B-	B-	B3	B-	B-	B-	6
CCC+	CCC+	CCCH	CCC+	CCC+	Caa1	CCC+	CCC+	CCC+	5
CCC	CCC	CCC	CCC	CCC	Caa2	CCC	CCC	CCC	4
CCC-	CCC-	CCCL	CCC-	CCC-	Caa3	CCC-	CCC-	CCC-	3
CC	CC	CC	CC	CC	Ca	CC	CC	CC	2
C	C	C		C	C	C	CC	C	1
DDD				DDD		DDD		SD	1
DD				DD		DD			1
D	D	D	D	D		D	D	D	1
				RD		RD			1

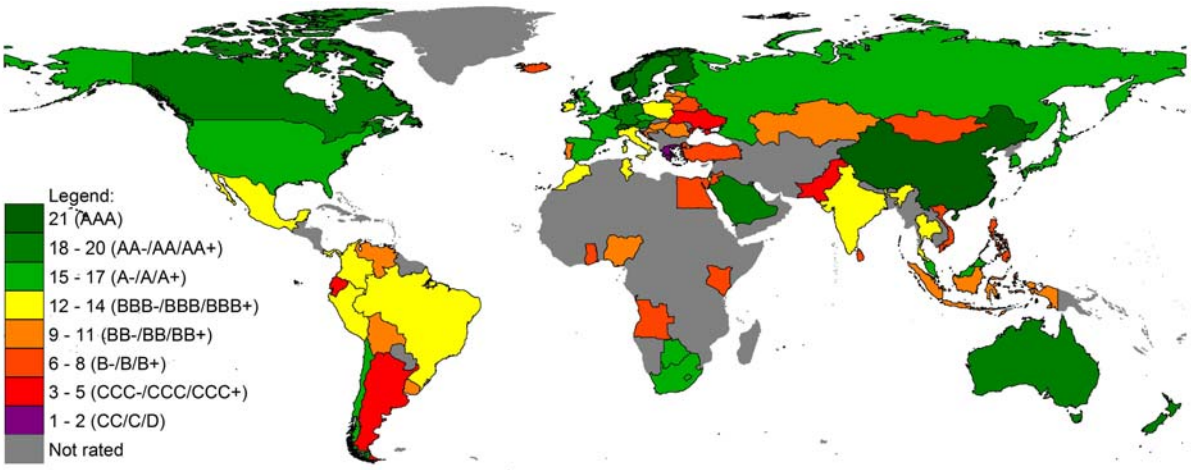
Sources: Rating scales from company webpages, except DBRS and Feri. DBRS and Feri scales were obtained from the agencies via personal e-mail communication.

Appendix II, A1: Sovereign ratings by agency and country (as of 28 June 2013)

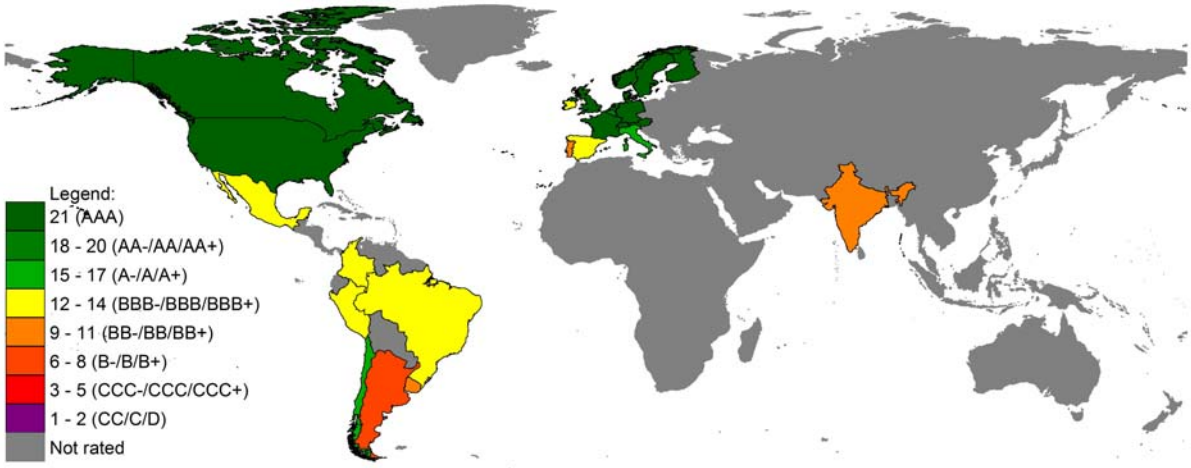
(a) CI (Cyprus/Kuwait)



(b) Dagong (China)

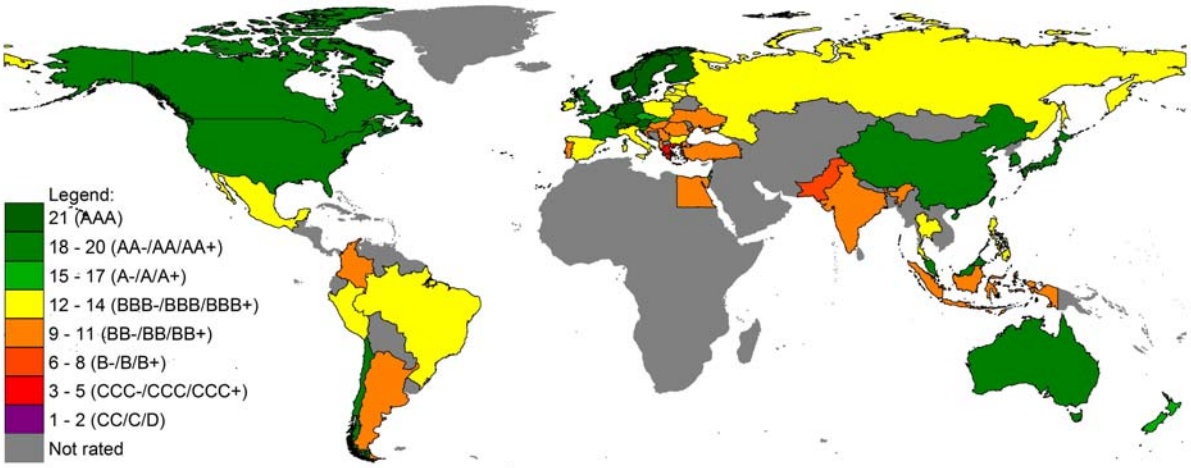


(c) DBRS (Canada)

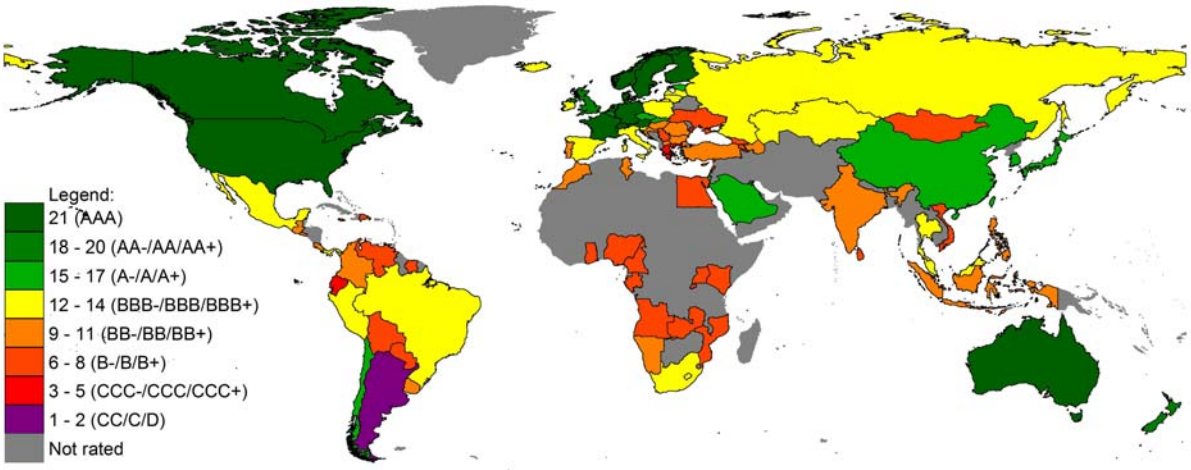


Appendix II, A1 (continued): Sovereign ratings by agency and country (as of 28 June 2013)

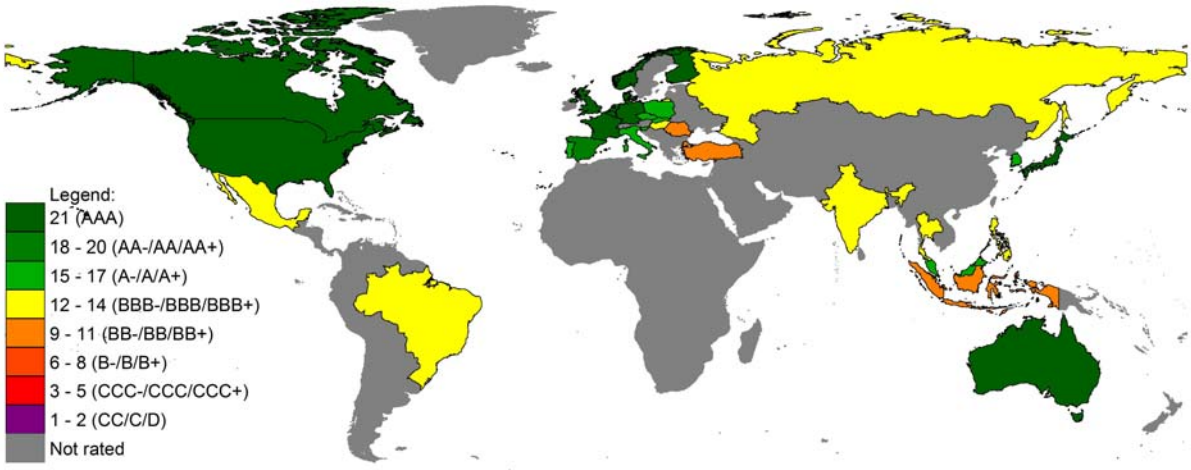
(d) Feri (Germany)



(e) Fitch (USA/France)

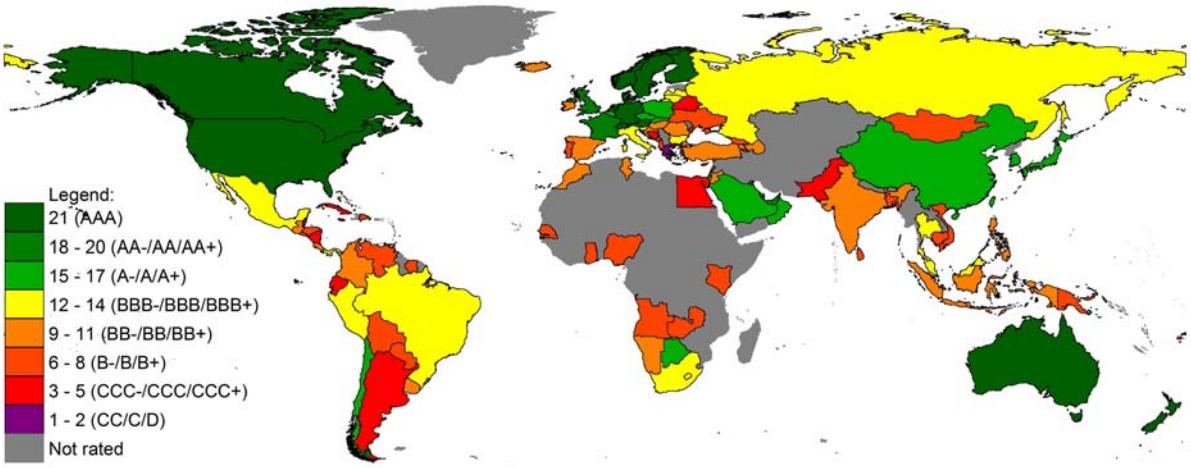


(f) JCR (Japan)

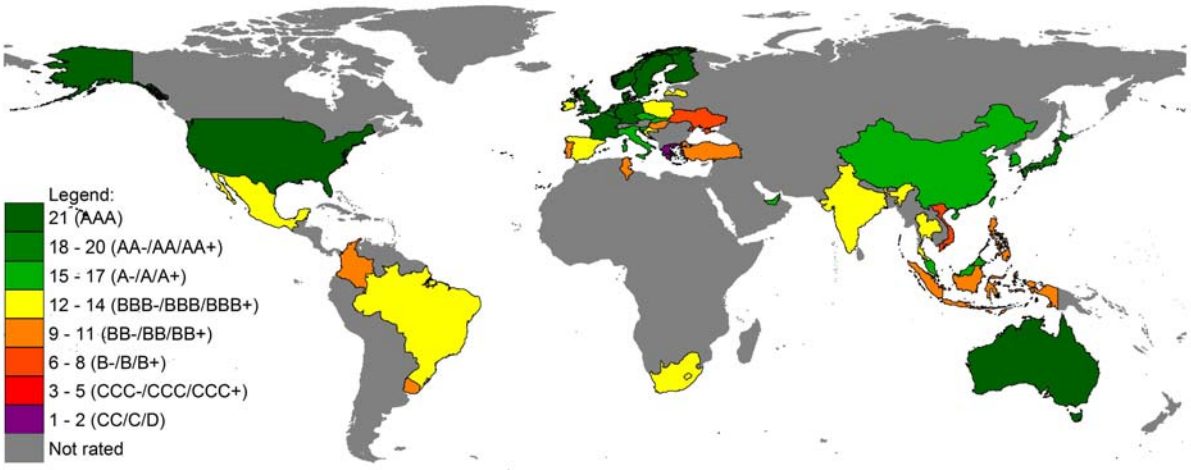


Appendix II, A1 (continued): Sovereign ratings by agency and country (as of 28 June 2013)

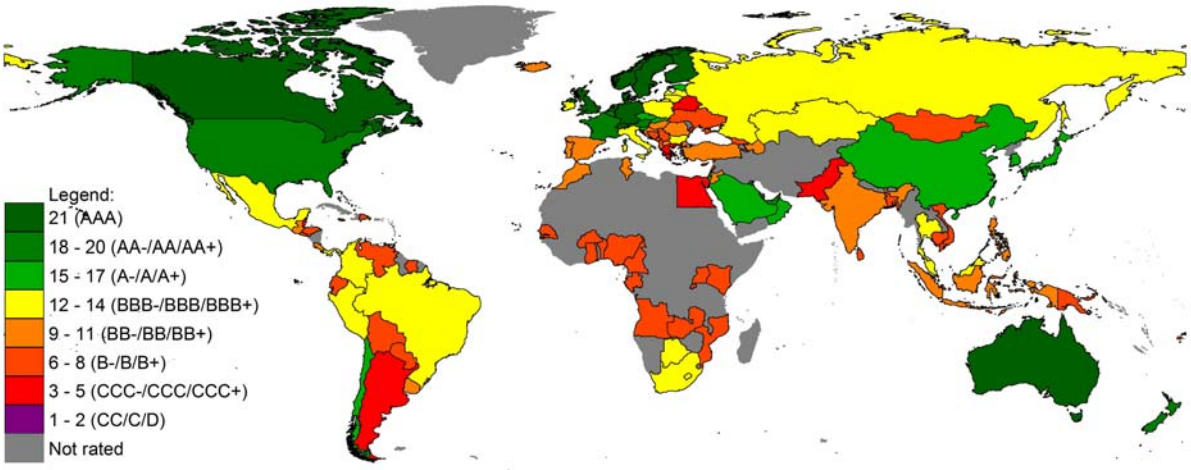
(g) Moody's (USA)



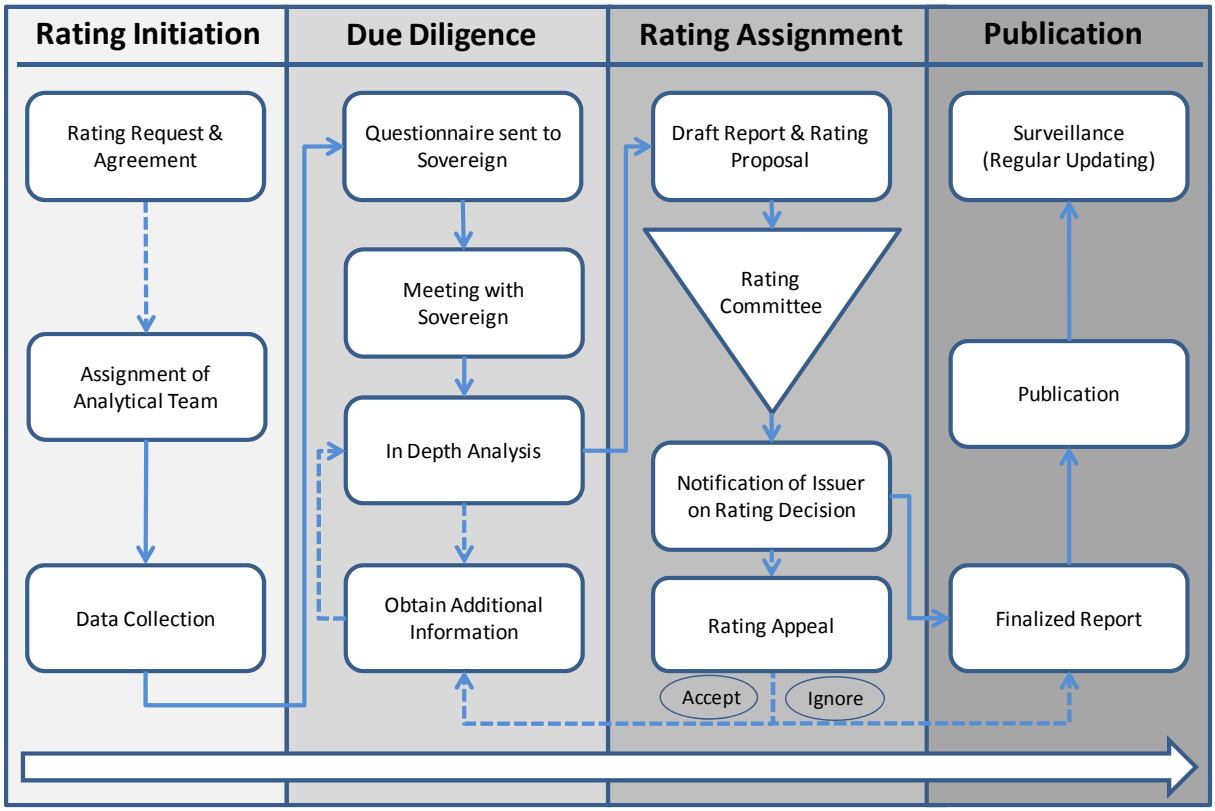
(h) R&I (Japan)



(i) S&P (USA)



Appendix II, A2: Schematic illustration of the rating process



Appendix II, A3: Comparison of credit rating process (sovereign ratings)

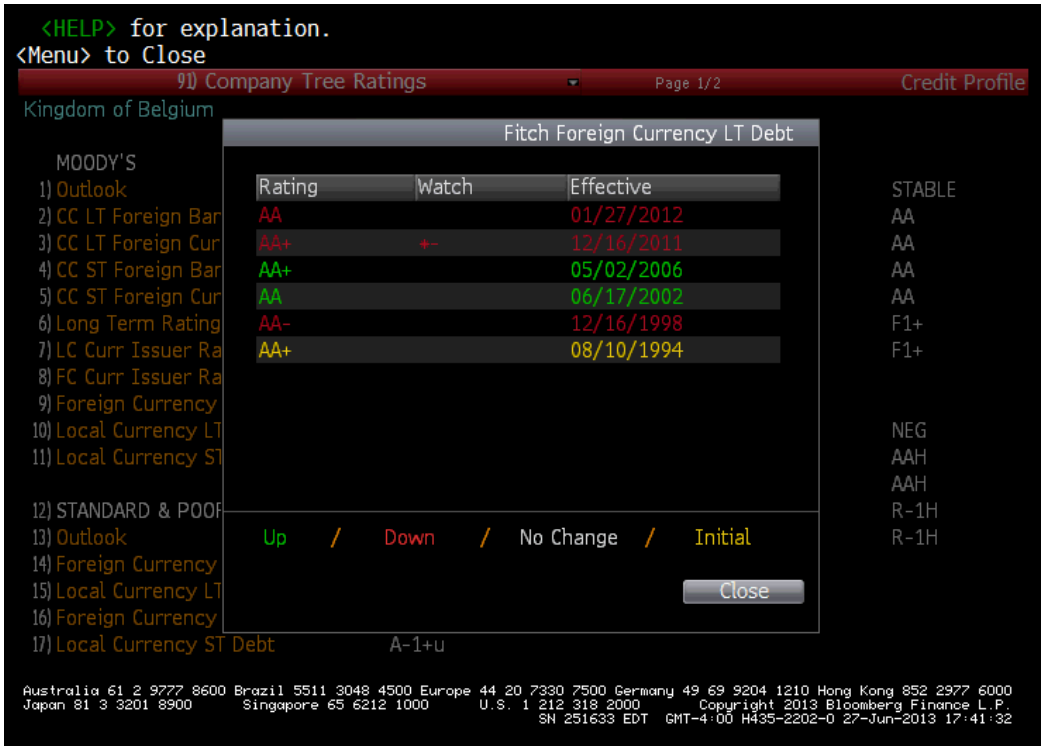
Agency	**CI**	**Dagong**	**DBRS**	**Feri**	**Fitch**	**JCR**	**Moody's**	**R&I**	**S&P**
Home country (location)	Cyprus	China	Canada	Germany	USA	Japan	USA	Japan	USA
Home country (ownership)	Kuwait	China	Canada	Germany	France	Japan	USA	Japan	USA
Does the agency provide - solicited ratings?	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes
- unsolicited rating?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
How long does a typical rating process take?	Unspecified	Unspecified	No information	10-30 days	30-60 days	60 days	60-90 days	Unspecified	30-45 days
How many analysts are involved in the rating process (per sovereign rating)?	One analyst	Project team (several analysts)	One analyst	One/two analysts and compliance officer	Two analysts	Two analysts	One analyst	Several analysts	Two analysts
Does the agency collect and analyze publically available data?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Does the agency make an official cooperation offer to the sovereign?	Yes	Yes	Yes*	No	Yes	Yes*	Yes	Yes*	Yes
Does an interview with the sovereign take place?	No*	No*	Yes*	No*	Yes*	Yes*	Yes*	Yes*	Yes*
Does the lead analyst submit a rating proposal to the rating committee?	Yes	No	Yes	Yes	No	Yes	Yes	Yes	Yes
Who is part of the rating committee responsible for the final rating decision?	Several analysts and chairperson	Project team, external experts and compliance officer	No information	Three or four	At least five analysts and senior director	At least four	Unspecified	Several analysts and chairperson	Five to seven analysts and chairperson
Are the issuers notified prior to the publication of the rating?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	Yes
Can sovereigns appeal against a rating publication?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	Yes
Surveillance: Is there a regular updating interval?	Unspecified	At least annually	Monthly	Monthly	At least annually	Monthly	Quarterly	Unspecified	Unspecified
Who initiates an update?	Lead analyst	Project team	Surveillance analysts	Lead analyst(s)& rating committee	All analysts	Unspecified	Lead analyst	Lead analyst	All analysts

Sources: Personal communication with Moody's Client Services (28 February 2013, 4 March 2013, 22 March 2013, 25 March 2013), Feri EuroRating Services AG (14 March 2013, 21 March 2013), Dagong Global Credit Rating (5 April 2013, 3 June 2013), and internet research (see Appendix II, A22 for a detailed list of sources).
Note: *: for solicited ratings only

Appendix II, A4: Construction of the rating database

Data on sovereign ratings assigned by CI, Dagong, DBRS, JCR, Moody's, R&I, and S&P have been obtained from Bloomberg. Hence, everybody with access to Bloomberg can replicate the data easily. We downloaded the data in late September 2012 in the library at Princeton University and updated all information on June 28, 2013.[80]
The approach was the following:

(1) To access the data, we logged on to a Bloomberg terminal and typed "CSDR." The variables selected are *Foreign Long Term* for CI, *FC LT Sovereign Ratings* for Dagong, *Foreign Currency LT Debt* for DBRS, JCR, Moody's and S&P, and *Foreign Curr Issuer Rtg* for R&I. We followed Bloomberg and collected Moody's foreign currency issuer rating if Moody's had not assigned a foreign-currency debt rating to a country. We took screenshots for each page displaying sovereign ratings.



Example of screenshot for Fitch Foreign Currency Long Term Debt ratings.

[80] The ratings from Feri and Fitch have been obtained from the companies directly.

2) Using these screenshots, two student assistants entered the letter-ratings into a database. The double coding was used to identify and correct typing errors.

3) The three-letter ratings were translated to numerical values according to the 21-point scale presented in Appendix II, A0.

4) We checked the data for potential errors, for example by examining rating changes by more than two steps. Two obvious mistakes in the R&I data from Bloomberg have been corrected after e-mail correspondence with the agency's chief analyst.

Appendix II, A5: Date of first and last rating assigned to sovereigns by agency (01/1990-06/2013)

Country	CI	Dagong	DBRS	Feri	Fitch	JCR	Moody's	R&I	S&P
Albania				06/1999-06/2013			11/2010-06/2013		04/2010-06/2013
Andorra									05/2003-06/2013
Angola		05/2013-06/2013			05/2010-06/2013		05/2010-06/2013		05/2010-06/2013
Argentina		07/2010-06/2013	09/2007-06/2013	06/1999-06/2013	05/1997-06/2013		01/1990-06/2013		08/1993-06/2013
Armenia					05/2006-06/2013		07/2006-06/2013		
Aruba					04/2002-06/2013				05/2008-06/2013
Australia		07/2010-06/2013		06/1999-06/2013	01/1996-06/2013	05/1998-06/2013	01/1990-06/2013	12/1999-06/2013	01/1990-06/2013
Austria		10/2010-06/2013	06/2011-06/2013	06/1999-06/2013	08/1994-06/2013		01/1990-06/2013	11/1999-09/2005	01/1990-06/2013
Azerbaijan					07/2000-06/2013		09/2006-06/2013		12/2008-06/2013
Bahamas							04/1997-06/2013		12/2003-06/2013
Bahrain	06/2008-06/2013				02/2000-06/2013		08/2002-06/2013		07/2002-06/2013
Bangladesh							04/2010-06/2013		04/2010-06/2013
Barbados							12/1994-06/2013		12/1999-06/2013
Belarus		04/2012-06/2013					07/2010-06/2013		08/2007-06/2013
Belgium		07/2010-06/2013	11/2011-06/2013	06/1999-06/2013	08/1994-06/2013	04/1998-06/2013	01/1990-06/2013	12/2000-06/2013	12/1992-06/2013
Belize							01/1999-06/2013		08/2000-06/2013
Benin					09/2004-01/2012				12/2003-06/2013
Bermuda					08/1994-06/2013		07/2010-06/2013		06/1995-06/2013
Bolivia		05/2013-06/2013			03/2004-06/2013		05/1998-06/2013		07/1998-06/2013
Bosnia & Herzegovina							04/2006-06/2013		12/2008-06/2013
Botswana		04/2013-06/2013					03/2001-06/2013		04/2001-06/2013
Brazil		07/2010-06/2013	07/2006-06/2013	06/1999-06/2013	12/1994-06/2013	06/2007-06/2013	01/1990-06/2013	04/1998-06/2013	11/1994-06/2013
Bulgaria				06/1999-06/2013	04/1998-06/2013	10/2002-06/2011	09/1996-06/2013		11/1998-06/2013
Burkina Faso									03/2004-06/2013
Cambodia	07/2010-06/2013						05/2007-06/2013		04/2007-06/2013
Cameroon					09/2003-06/2013				11/2003-06/2013
Canada		07/2010-06/2013	01/2000-06/2013	06/1999-06/2013	08/1994-06/2013	07/2000-06/2013	01/1990-06/2013	03/2000-09/2012	01/1990-06/2013
Cape Verde					08/2003-06/2013				12/2008-06/2013
Cayman Islands		02/2012-06/2013					11/2009-06/2013		
Chile		07/2010-06/2013	05/2006-06/2013	06/1999-06/2013	11/1994-06/2013		05/1999-06/2013		12/1992-06/2013
China	10/2007-06/2013	07/2010-06/2013		06/1999-06/2013	12/1997-06/2013		01/1990-06/2013	04/1998-06/2013	12/1992-06/2013
Colombia		03/2012-06/2013	12/2006-06/2013	06/1999-06/2013	08/1994-06/2013		08/1993-06/2013	12/2012-06/2013	06/1993-06/2013
Cook Is									01/1998-06/2013
Costa Rica					05/1998-06/2013		05/1997-06/2013		07/1997-06/2013
Croatia		04/2012-06/2013		06/1999-06/2013	01/1997-06/2013		01/1997-06/2013	04/1998-06/2013	01/1997-06/2013
Cuba							07/1999-06/2013		
Cyprus	11/2007-06/2013				02/2002-06/2013		01/1998-06/2013		02/1994-06/2013
Czech Rep	09/2008-06/2013	11/2011-06/2013		06/1999-06/2013	08/1995-06/2013	05/2001-06/2013	06/1998-06/2013	04/1998-06/2013	07/1993-06/2013
Denmark		07/2010-06/2013	09/2012-06/2013	06/1999-06/2013	08/1994-06/2013	10/2001-06/2013	01/1990-06/2013	05/2001-06/2013	01/1990-06/2013
Dominican Rep					08/2003-06/2013		05/2001-06/2013		02/1997-06/2013
Ecuador		07/2010-06/2013			11/2002-06/2013		07/1997-06/2013		07/2000-06/2013
Egypt	09/2007-06/2013	07/2010-06/2013		06/1999-06/2013	08/1997-06/2013		07/2001-06/2013		01/1997-06/2013
El Salvador					09/1996-06/2013		02/2002-06/2013		08/1996-06/2013
Estonia		07/2010-06/2013		06/1999-06/2013	09/1997-06/2013		06/2002-06/2007		12/1997-06/2013
Fiji							08/2006-06/2013		08/2006-06/2013
Finland		12/2010-06/2013	08/2012-06/2013	06/1999-06/2013	08/1994-06/2013	02/2001-06/2013	01/1990-06/2013	05/2001-06/2013	01/1990-06/2013
France		07/2010-06/2013	05/2011-06/2013	06/1999-06/2013	08/1994-06/2013	11/2000-06/2013	02/1992-06/2013	03/2000-06/2013	01/1990-06/2013
Gabon					10/2007-06/2013				11/2007-06/2013
Gambia					11/2002-07/2007				

Country	CI	Dagong	DBRS	Feri	Fitch	JCR	Moody's	R&I	S&P
Georgia					07/2007-06/2013		10/2010-06/2013		12/2005-06/2013
Germany		07/2010-06/2013	06/2011-06/2013	06/1999-06/2013	08/1994-06/2013	11/2000-06/2013	04/1993-06/2013	03/2000-06/2013	01/1990-06/2013
Ghana		04/2013-06/2013			12/2003-06/2013		12/2012-06/2013		09/2003-06/2013
Greece	02/2010-06/2013	07/2010-06/2013		06/1999-06/2013	11/1995-06/2013		05/1994-06/2013	06/2000-06/2013	12/1992-06/2013
Grenada									03/2002-06/2013
Guatemala					02/2006-06/2013		08/1997-06/2013		10/2001-06/2013
Honduras							03/2013-06/2013		10/2008-06/2013
Hong Kong (China)	11/2007-06/2013	10/2010-06/2013		06/1999-06/2013	08/1994-06/2013	04/1998-06/2013	07/2004-06/2013	04/1998-06/2013	12/1992-06/2013
Hungary	11/2008-06/2013	07/2010-06/2013		06/1999-06/2013	04/1996-06/2013	11/1999-06/2013	12/1993-06/2013	11/2000-06/2013	04/1992-06/2013
Iceland		07/2010-06/2013			02/2000-06/2013		01/1990-06/2013	06/2007-09/2012	12/1992-06/2013
India	02/2007-06/2013	07/2010-06/2013	06/2007-06/2013	06/1999-06/2013	03/2000-06/2013	06/1998-06/2013	07/1999-06/2013	06/1998-06/2013	12/1992-06/2013
Indonesia	02/2009-06/2013	07/2010-06/2013		06/1999-06/2013	06/1997-06/2013	10/2002-06/2013	03/1994-06/2013	04/1998-06/2013	12/1992-06/2013
Iran	02/2010-06/2013				05/2002-04/2008				
Ireland		12/2010-06/2013	07/2010-06/2013	06/1999-06/2013	10/1994-06/2013		01/1990-06/2013	04/1998-06/2013	01/1990-06/2013
Isle of Man							04/2003-06/2013		02/2000-06/2013
Israel		07/2010-06/2013		06/1999-06/2013	11/1995-06/2013		12/1995-06/2013		12/1992-06/2013
Italy		07/2010-06/2013	02/2011-06/2013	06/1999-06/2013	08/1994-06/2013	04/1998-06/2013	01/1990-06/2013	04/1998-06/2013	12/1992-06/2013
Jamaica					08/2006-06/2013		03/1998-06/2013		11/1999-06/2013
Japan		07/2010-06/2013		06/1999-06/2013	08/1994-06/2013	10/2000-06/2013	12/2002-06/2013	03/2000-06/2013	01/1990-06/2013
Jordan	10/2008-06/2013	05/2013-06/2013					11/2010-06/2013		10/1995-06/2013
Kazakhstan		07/2010-06/2013			11/1996-06/2013		11/1996-05/2007		11/1996-06/2013
Kenya		12/2010-06/2013			12/2007-06/2013		11/2012-06/2013		09/2006-06/2013
Korea	06/2007-06/2013	07/2010-06/2013		06/1999-06/2013	06/1996-06/2013	04/1998-06/2013	04/1998-06/2013	04/1998-06/2013	01/1990-06/2013
Kuwait	04/2007-06/2013	05/2012-06/2013			12/1995-06/2013		07/1999-06/2013		10/1997-06/2013
Latvia	11/2008-06/2013	10/2010-06/2013		06/1999-06/2013	06/1998-06/2013		08/1999-06/2013	09/2008-06/2013	01/1997-06/2013
Lebanon					02/1997-06/2013		02/1997-06/2013	04/1998-01/2011	02/1997-06/2013
Lesotho					09/2002-06/2013				
Libya					05/2009-04/2011				03/2009-03/2011
Liechtenstein									12/1996-06/2013
Lithuania		03/2011-06/2013		06/1999-06/2013	01/1997-06/2013		09/1996-06/2013		06/1997-06/2013
Luxembourg		07/2010-06/2013			08/1994-06/2013		07/1999-06/2013	12/2000-09/2012	04/1994-06/2013
Macau	09/2008-06/2013	10/2010-06/2013			05/2013-06/2013		07/1999-06/2013		
Macedonia					11/2005-06/2013	09/2007-01/2012			08/2005-06/2013
Malawi					05/2003-08/2009				
Malaysia	09/2006-06/2013	07/2010-06/2013		06/1999-06/2013	08/1998-06/2013	10/2001-06/2013	01/1990-06/2013	04/1998-06/2013	09/1990-06/2013
Mali					04/2004-12/2009				05/2004-07/2008
Malta					07/1996-06/2013		07/2000-06/2013		03/1994-06/2013
Mauritius		07/2012-06/2013					03/1996-10/2000		
Mexico		07/2010-06/2013	07/2006-06/2013	06/1999-06/2013	08/1995-06/2013	06/1998-06/2013	02/1991-06/2013	11/2000-06/2013	07/1992-06/2013
Moldova					07/1998-12/2009		01/1997-09/2009		
Mongolia		07/2010-06/2013			07/2005-06/2013		10/2005-06/2013		12/1999-06/2013
Montenegro							03/2008-06/2013		12/2004-06/2013
Montserrat									10/2004-06/2013
Morocco	10/2007-06/2013	10/2010-06/2013			04/2007-06/2013		07/1999-06/2013	05/2000-12/2005	03/1998-06/2013
Mozambique					07/2003-06/2013				07/2004-06/2013
Namibia					12/2005-06/2013		09/2011-06/2013		
Netherlands		07/2010-06/2013	05/2011-06/2013	06/1999-06/2013	08/1994-06/2013	02/2001-06/2013	02/2012-06/2013	12/2000-06/2013	12/1992-06/2013
New Zealand		07/2010-06/2013		06/1999-06/2013	03/2002-06/2013		01/1990-06/2013	12/1999-09/2012	01/1990-06/2013
Nicaragua							07/1999-06/2013		
Nigeria		07/2010-06/2013			01/2006-06/2013		11/2012-06/2013		02/2006-06/2013
Norway		07/2010-06/2013	03/2012-06/2013	06/1999-06/2013	03/1995-06/2013	02/2000-06/2013	01/1990-06/2013	05/2001-06/2013	01/1990-06/2013

Country	CI	Dagong	DBRS	Feri	Fitch	JCR	Moody's	R&I	S&P
Oman	04/2008-06/2013						07/1999-06/2013		02/1996-06/2013
Pakistan	10/2008-06/2013	07/2010-06/2013		06/1999-06/2013			11/1994-06/2013		11/1994-06/2013
Panama		07/2012-06/2013			09/1998-06/2013		01/1997-06/2013		01/1997-06/2013
Papua New Guinea					01/1999-01/2010		07/1999-06/2013		01/1999-06/2013
Paraguay					01/2013-06/2013		07/1999-06/2013		10/1995-06/2013
Peru		10/2010-06/2013	10/2007-06/2013	06/1999-06/2013	10/1999-06/2013		07/1999-06/2013		12/1997-06/2013
Philippines	12/2006-06/2013	07/2010-06/2013		06/1999-06/2013	07/1999-06/2013	04/1998-06/2013	07/1993-06/2013	04/1999-06/2013	06/1993-06/2013
Poland	12/2007-06/2013	07/2010-06/2013		06/1999-06/2013	10/1995-06/2013	05/2002-06/2013	06/1995-06/2013	07/2000-06/2013	06/1995-06/2013
Portugal		07/2010-06/2013	11/2010-06/2013	06/1999-06/2013	08/1994-06/2013	01/2000-06/2013	01/1990-06/2013	06/2000-06/2013	12/1992-06/2013
Qatar	01/2008-06/2013						09/1999-06/2013		02/1996-06/2013
Romania	11/2008-06/2013	07/2010-06/2013		06/1999-06/2013	03/1996-06/2013	04/1998-06/2013	06/1997-06/2013		04/1997-06/2013
Russia		07/2010-06/2013		06/1999-06/2013	10/1996-06/2013	03/2007-06/2013	11/1996-06/2013		10/1996-06/2013
Rwanda					12/2006-06/2013				12/2011-06/2013
Saint Vincent and the Grenadines							03/2008-06/2013		
San Marino					01/2001-06/2013				
Saudi Arabia	02/2008-06/2013	07/2010-06/2013			11/2004-06/2013		07/1999-06/2013		07/2003-06/2013
Senegal							03/2011-06/2013		12/2000-06/2013
Serbia				01/2005-06/2013	05/2005-06/2013				11/2004-06/2013
Seychelles					02/2010-06/2013				09/2006-08/2009
Singapore	08/2002-06/2013	07/2010-06/2013		06/1999-06/2013	11/1998-06/2013	04/1998-06/2013	07/1999-06/2013	04/1998-06/2013	12/1992-06/2013
Slovak Republic	09/2008-06/2013			06/1999-06/2013	08/1996-06/2013	03/2001-06/2013	05/1995-06/2013	04/1998-06/2013	02/1994-06/2013
Slovenia	12/2006-06/2013			06/1999-06/2013	05/1996-06/2013	04/2005-06/2013	05/1996-06/2013	11/2000-06/2013	05/1996-06/2013
South Africa	01/2006-06/2013	07/2010-06/2013			09/1994-06/2013		10/1994-06/2013	04/1998-06/2013	10/1994-06/2013
Spain		07/2010-06/2013	10/2010-06/2013	06/1999-06/2013	08/1994-06/2013	06/2006-06/2013	01/1990-06/2013	07/1998-06/2013	01/1990-06/2013
Sri Lanka	12/2006-06/2013	03/2011-06/2013			12/2005-06/2013		07/2011-06/2013		12/2005-06/2013
Suriname					06/2004-06/2013		02/2004-06/2013		11/1999-06/2013
Sweden		10/2010-06/2013	04/2012-06/2013	06/1999-06/2013	08/1994-06/2013		01/1990-06/2013	03/1999-06/2013	01/1990-06/2013
Switzerland		07/2010-06/2013	07/2011-06/2013	06/1999-06/2013	08/1994-06/2013		01/1990-06/2013		12/1992-06/2013
Taiwan	11/2003-06/2013	10/2010-06/2013		06/1999-06/2013	11/2001-06/2013		07/1999-06/2013	12/1998-06/2013	12/1992-06/2013
Thailand	04/2005-06/2013	07/2010-06/2013		06/1999-06/2013	05/1998-06/2013	04/1998-06/2013	01/1990-06/2013	04/1998-06/2013	01/1990-06/2013
Trinidad & Tobago							02/1993-06/2013		03/1996-06/2013
Tunisia	09/2004-06/2013	10/2010-06/2013			09/1995-06/2013		04/2003-06/2013	04/1998-06/2013	04/1997-06/2013
Turkey	07/2005-06/2013	07/2010-06/2013		06/1999-06/2013	08/1994-06/2013	04/1998-06/2013	05/1992-06/2013	09/2012-06/2013	04/1994-06/2013
Turkmenistan					01/1998-02/2005		07/1999-09/2010		
Uganda					03/2005-06/2013				12/2008-06/2013
Ukraine		07/2010-06/2013		06/1999-06/2013	06/2001-06/2013		02/1998-06/2013	04/1998-06/2013	12/2001-06/2013
United Arab Emirates	05/2007-06/2013						10/2000-06/2013	01/2011-06/2013	07/2007-06/2013
United Kingdom		07/2010-06/2013	07/2010-06/2013	06/1999-06/2013	08/1994-06/2013	11/2000-06/2013	01/1990-06/2013	03/2000-06/2013	01/1990-06/2013
United States		07/2010-06/2013	09/2011-06/2013	06/1999-06/2013	08/1994-06/2013	11/2000-06/2013	01/1990-06/2013	03/2000-06/2013	09/1991-06/2013
Uruguay		12/2010-06/2013	02/2008-06/2013		01/1995-06/2013		10/1993-06/2013	04/1998-06/2013	02/1994-06/2013
Venezuela		07/2010-06/2013			09/1997-06/2013		01/1990-06/2013		01/1990-06/2013
Viet Nam	09/2008-06/2013	07/2010-06/2013			06/2002-06/2013		04/1997-06/2013	02/2008-06/2013	05/2002-06/2013
Zambia					03/2011-06/2013		11/2012-06/2013		03/2011-06/2013

Note: Each cell lists the first end last month in which a particular agency has assigned a rating to a particular country. There may be gaps in between.

Appendix II, A6: Pair-wise correlation of sovereign ratings from different agencies (1990-2013)

	CI	Dagong	DBRS	Feri	Fitch	JCR	Moody's	R&I	S&P
CI	1.000								
Dagong	0.918	1.000							
DBRS	.	0.902	1.000						
Feri	0.784	0.888	0.851	1.000					
Fitch	0.957	0.932	0.977	0.794	1.000				
JCRA	0.974	0.846	0.965	0.841	0.971	1.000			
Moody's	0.956	0.937	0.975	0.805	0.981	0.962	1.000		
R&I	0.927	0.918	0.982	0.837	0.973	0.981	0.972	1.000	
S&P	0.966	0.938	0.986	0.815	0.987	0.970	0.981	0.971	1.000

Note: No correlation between CI and DBRS is displayed since there is not enough overlap between the sovereigns rated by both agencies.

Appendix II, A7: Analytical key factors of sovereign rating assignments

Agency / Indicators	**CI** Cyprus (Kuwait)	**Dagong** China	**DBRS** Canada	**Feri** Germany	**Fitch** USA (France)	**JCR** Japan	**Moody's** USA	**R&I** Japan	**S&P** USA
Domestic economic performance	-Economic growth prospects	-Economic strength -Economic structure -Total GDP	-Historical growth and prospects -Inflation -Demographics and social structure	-Economic growth -Price stability	-Demographic, educational and structural factors -Labor market analysis -Structure of output and trade -Dynamism of the private sector -Balance of supply and demand	-Industrial structure -Fundamentals of economic development -Growth potential -Economic achievements	-GDP per capita -Diversification and size -Long-term trends	-Economic fundamentals -Growth	-Economic structure and growth prospects
Political and institutional performance	-Level of political risk	-Political risks -Institutional strength	-Political environment -Government policy management	-Rule of law -Economic freedom -Political condition	-Foreign investment policy -Policies and the state -Macroeconomic policy	-Political and social stability -Policy effectiveness and recognition -State of governance	-Rule of law -Governance -Transparency -Regulatory developments	-Socio-political fundamentals -Policy management capacity	-Institutional effectiveness -Political risks
Financial stability and fiscal performance	-Sustainability of public finances	-Financial risks -Governments debt repayment capability -Banking system	-Expenditure -Revenues -Fiscal balance -Budget planning -Monetary policy -Financial system stability	-Public finance -Capital market structure	-Banking and finance -Analysis of medium-term growth constraints	-Trends in the fiscal balance -Financial system -Framework of financial regulations and supervision	-Government balance sheet	-Fiscal conditions -Funding structure	-Fiscal performance and flexibility -Debt burden -Monetary flexibility
External performance	-Country's external growth position	-International reserves -External debt -Short-term foreign debt -Currency reserve system	-International economic integration -External debt & liquidity	-Net external position -Current account -Capital account	-External assets -External liabilities -International position -Balance of payments	-External debt structure -Trends in balance of payments -Management of foreign exchange reserves	-Financial risk (external debts) -Susceptibility to external risk -Balance of payments		-External liquidity -International investment position

Sources: Personal communication with Moody's Client Services (28 February 2013, 4 March 2013, 22 March 2013, 25 March 2013), Feri EuroRating Services AG (14 March 2013, 21 March 2013), Dagong Global Credit Rating (5 April 2013, 3 June 2013), and internet research (see Appendix II, A22 for detailed list of sources)

Appendix II, A8: Variables, definitions and sources

Variable	Definition	Source
Dependent variable		
Sovereign rating	Sovereign rating on a 21-point scale (see Appendix II, A0 and A4 for details; monthly average)	Bloomberg (via terminal); Feri (via e-mail); Fitch (http://www.fitchratings.com)
Variables of interest		
Same country	1 if rated country is the country where the headquarters of the rating agency is located [alternative definition: 1 if rated country is the country of the rating agency's main shareholders]	Own construction
Export interests	Home-country exports (in % of total home-country exports), 3-year average, lag	UN Comtrade via WITS (https://wits.worldbank.org/)
Bank exposure	Overall claims of home-country banks to the rated country (in % of home country's total foreign claims; all sectors; private and public banks; guarantees extended and credit commitments; all on ultimate risk basis), 1-year average, lag	Bank for International Settlements (http://www.bis.org/statistics/consstats.htm)
Geopolitical alignment (UN)	Voting alignment between home country and rated country in the United Nations General Assembly (in % of total votes), 3-year average, lag	Strezhnev and Voeten (2012)
US military interests (aid)	Military aid provided by the United States to the rated country (in % of US total military aid), 3-year average, lag	USAID (http://gbk.eads.usaidallnet.gov)
Common language	1 if common official or primary language in home country and rated country	CEPII (Mayer and Zignago 2011)
Cultural distance (language)	Language dissimilarity between home and rated country (values between 0 and 100)	Kolo (2012)
Cultural distance (ethno-racial)	Ethno-racial dissimilarity between home and rated country (values between 0 and 100)	Kolo (2012)
Control variables: Domestic Economic Performance		
GDP per capita*#	Log GDP per capita (constant 2000 US$), 1-year average, lag	World Development Indicators 2014 (http://data.worldbank.org/)
GDP growth*#	GDP growth (annual %), 3-year average, lag	World Development Indicators 2014 (http://data.worldbank.org/)
GDP growth squared#	GDP growth (annual %) squared, 3-year average, lag	World Development Indicators 2014 (http://data.worldbank.org/)
Inflation*#	Inflation, consumer prices (annual %), 3-year average, lag	World Development Indicators 2014 (http://data.worldbank.org/)
Natural resources*	Sum of oil rents, natural gas rents, coal rents (hard and soft), mineral rents, and forest rents (in % of GDP), 3-year average lag	World Development Indicators 2014 (http://data.worldbank.org/)
Population	Log total population size, 1-year average, lag	World Development Indicators 2014 (http://data.worldbank.org/)
Control variables: Financial Stability and Fiscal Performance		
Change in government debt#	Change in gross government debt-to-GDP ratio (in %), 3-year average, lag	IMF Historical Public Debt Database 2013 (Abbas et al. 2010)
Government debt	Gross government debt-to-GDP ratio (in %), 3-year average, lag	IMF Historical Public Debt Database 2013 (Abbas et al. 2010)
Default (since 1970)#	1 if country has experienced a sovereign debt crisis or restructuring since 1970	Laeven and Valencia (2012)
Default (last 5 years)*	1 if country has experienced a sovereign debt crisis or restructuring in the last five years	Laeven and Valencia (2012)

Control variables: External Performance		
Trade openness*	Sum of exports and imports of goods and services (in % of GDP), 3-year average lag	World Development Indicators 2014 (http://data.worldbank.org/)
Current account balance*#	Sum of net exports of goods and services, net primary income, and net secondary income (% of GDP), 3-year average, lag	World Development Indicators 2012, 2014 (http://data.worldbank.org/)
External debt*#	Debt owed to nonresidents repayable in foreign currency, goods, or services (% of GDP), 3-year average, lag (all missing values set to zero if rated country is at least an upper-middle-income country, see Afonso 2003 and Hill et al. 2010 for a similar approach)	World Development Indicators 2012, 2014 (http://data.worldbank.org/)

Appendix II, A8 (continued): Variables, definitions and sources

Control variables: Political and Institutional Performance		
Rule of law	Assessment of both the strength and impartiality of the legal system and of popular observance of the law on a 6-point scale, 1-year average, lag	International Country Risk Guide (http://www.prsgroup.com/ICRG.aspx)
Polity*	Regime authority on a 21-point scale ranging from -10 (hereditary monarchy) to +10 (consolidated democracy), 3-year average, lag	Polity IV dataset version 2012 (Marshall et al. 2013)
Election*	1 if presidential elections (for presidential or assembly-elected systems) or parliamentary elections (for parliamentary systems) were held in rated country during the last 12 months (DPI variables DATEEXEC, DATELEG and SYSTEM)	Database of Political Institutions 2012 (Beck et al. 2001)
Years in office*	Number of years the chief executive has been in office as of January 1st, lag (DPI variable YRSOFFC; some errors corrected), 1-year average, lag	Database of Political Institutions 2012 (Beck et al. 2001)
Left government*	1 if the chief executive's party is defined as communist, socialist, social democratic or left-wing, 1-year average, lag	Database of Political Institutions 2012 (Beck et al. 2001)
Absence of internal conflict	Assessment of both political violence in the country and its actual or potential impact on governance on a 12-point scale, 1-year average, lag	International Country Risk Guide (http://www.prsgroup.com/ICRG.aspx)
Absence of external conflict	Assessment of the risk to the incumbent government from foreign action on a 12-point scale, 1-year average, lag	International Country Risk Guide (http://www.prsgroup.com/ICRG.aspx)
Absence of military in Politics	Assessment of the degree of military participation in politics on a 6-point scale, 1-year average, lag	International Country Risk Guide (http://www.prsgroup.com/ICRG.aspx)
Euro area	1 if rated country is a member of the euro area	Own construction

Other variables		
Office in rated country	1 if an agency has a subsidiary/office in a rated country	Own construction, gathered from company websites
Unsolicited ratings	1 if a rated country receives an unsolicited rating by S&P	S&P website (http://www.standardandpoors.com/)
Same legal origin		Treisman
Bilateral trust	Trust level of a representative citizen towards a randomly selected individual of the rated country (based on the following Eurobarometer question on a 4-point scale: "I would like to ask you a question about how much trust you have in people from various countries. For each, please tell me whether you have a lot of trust, some trust, not very much trust, or no trust at all.")	Yu et al. (2015)

Notes: * - variable included in Archer et al. (2007), # - variable included in Hill et al. (2010). The next page provides a detailed description of the computation of lagged moving averages of the explanatory variables.

Appendix II, A8 (continued): Variables, definitions and sources

Computation of moving averages: Our econometric specification contains explanatory variables in the form of lagged moving averages. By doing so, we model a rating methodology in which agencies update their belief over the course of a year by gradually incorporating new information. The "1-year average, lag," as indicated in the table above, refers to the moving average of each variable over the previous 12 months:

$$x_{j,i,t} = \frac{1}{12}\sum_{t=-12}^{-1} x_{i,j,t} \ ,$$

where x is the respective explanatory variable, j stands for the home country, i for the rated country, and t is time in monthly frequency in the format MM/YYYY. In cases where the observed variable is only available at a yearly frequency, it becomes a weighted average of the existing information and new information. This implies that, over the course of a year, the weight of "new" information gradually increases. We make the implicit assumption that the final, actual value published for a particular year is a reliable proxy for the information that was available to the agencies during that year, for example, from news and other information sources.

For example, assume an agency wants to assess the per-capita GDP of country i in July 2011. Per-capita GDP data by the World Bank is only available on a yearly basis, i.e., for the year 2010. However, within a year there will be new information that indicates whether a country's per-capita GDP has improved, stayed constant or deteriorated. Hence, it makes sense for an agency to assess per-capita GDP within a year as a weighted average of the verified information from 2010 and the updated but still unverified new information. We will thus compute the moving average of the "GDP p.c." variable for July 2011 as follows:

$$GDP\ p.c._{\cdot i,07/2011} = \frac{1}{12}\sum_{t=07/2010}^{06/2011} GDP\ p.c._{\cdot i,t} = \frac{1}{2}GDP\ p.c._{\cdot i,2010} + \frac{1}{2}GDP\ p.c._{\cdot i,2011}$$

We use a longer lag structure for more volatile variables such as GDP growth and inflation (as indicated by "3-year average, lag" in the table above). This means that we calculate the average over the last 36 months to cancel out pure business-cycle effects and

random short-term fluctuations that should not influence long-term debt repayment abilities (see Block and Vaaler 2004 for a similar approach). The formula does then becomes

$$x_{i,j,t} = \frac{1}{36}\sum_{t=-36}^{-1} x_{j,i,t}.$$

Appendix II, A9: Descriptive statistics

Variable	Observations	Mean	Std.dev.	Min.	Max.
Dependent variable					
Sovereign rating	75438	14.79	4.95	1	21
Variables of interest					
Same country	75438	0.02	0.14	0	1
Export interests	73919	1.47	3.48	0	76.37
Bank exposure	37949	1.47	4.38	0	70.49
Geopolitical alignment (UN)	75048	52.97	23.67	12.33	95.72
US military interests (aid)	36923	1.21	5.6	0	51.24
Common language	75438	0.16	0.37	0	1
Cultural distance (language)	75438	89.63	14.84	3.23	100
Cultural distance (ethno-racial)	75438	76.79	24.87	1.1	100
Control variables: Domestic Economic Performance					
GDP per capita (log)	75438	9.09	1.33	5.33	11.38
GDP growth	75438	3.57	2.92	-7.75	28.65
GDP growth squared	75438	21.26	31.67	0	820.8
Inflation	75438	0.05	0.07	-0.03	0.94
Natural resources	75438	6.03	10.53	0	65.37
Population (log)	75438	16.76	1.61	12.91	21.02
Control variables: Financial Stability and Fiscal Performance					
Change in government debt	75438	3.35	5.1	-62.19	50.07
Government debt	75438	51.71	30.81	0	233.11
Default (since 1970)	75438	0.3	0.46	0	1
Default (last 5 years)	75438	0.05	0.23	0	1
Control variables: External Performance					
Trade openness	75438	87.66	58.27	14.49	428.14
Current account balance	75438	0.02	7.31	-29.87	40.5
External debt	75438	0.19	0.28	0	1.71
Control variables: Political and Institutional Performance					
Rule of law	75438	4.34	1.28	1	6
Polity	75438	16.83	5.15	0	20
Election	75438	0.23	0.42	0	1
Years in office	75438	5.11	5.48	1	46
Left government	75438	0.33	0.46	0	1
Absence of internal conflict	75438	9.83	1.59	3.38	12
Absence of external conflict	75438	10.37	1.22	4	12
Absence of military in politics	75438	4.69	1.36	0	6
Euro area	75438	0.14	0.35	0	1
Other variables					
Office in rated country	75438	0.23	0.42	0	1
Unsolicited rating	75438	0.20	0.40	0	1
Bilateral trust	2334	2.75	0.20	2.36	2.99

Notes: This table shows descriptive statistics for the full sample used in Online Appendix II, A12, column 1. It contains data from January 1990 to June 2013.

Appendix II, A10: Correlation between sovereign rating and variables of interest (1990-2013)

	Correlation	Partial correlation (conditional on *GDP per capita*)	Partial correlation (conditional on *Government debt*)	Number of observations
Same country	0.16	2.06	6.29	110,298
Export interests	0.30	0.17	0.49	107,619
Bank exposure	0.32	0.12	0.44	53,896
Political alignment (UN)	-0.19	0.20	-0.54	107,924
US military interests (aid)	0.36	0.02	0.08	104,991
Common language	0.06	0.01	0.06	44,125
Cultural distance (language)	0.34	0.40	4.33	61,470
Cultural distance (ethno-racial)	0.07	0.64	0.77	110,298

Note: The full sample contains data from January 1990 to June 2013.

Appendix II, A11: Home bias in sovereign ratings (OLS, all agencies pooled, yearly averages)

	(1) Full sample	(2) GFC sample
(a) Same country	0.980**	1.713***
	[0.017]	[0.000]
(b) Export interests	0.018	0.029
	[0.290]	[0.132]
(c) Bank exposure	0.038*	0.044*
	[0.067]	[0.087]
(d) Geopolitical alignment (UN)	0.006	0.015**
	[0.310]	[0.018]
(e) US military interests (aid)	0.030	0.108***
	[0.240]	[0.007]
(f) Common language	0.734***	0.809***
	[0.001]	[0.002]
(g) Cultural distance (language)	-0.022***	-0.028***
	[0.000]	[0.000]
(h) Cultural distance (ethno-racial)	0.001	0.003
	[0.569]	[0.342]

Notes: This table shows the results when we replicate the OLS regressions of Table 3 (columns 1 and 2) with yearly data by collapsing the monthly data. The dependent variable is a country's sovereign rating on a 21-point scale. Each cell refers to a separate regression. The table displays only the coefficients on the respective variable of interest of each regression. All regressions contain the control variables as specified in Appendix II, A12, time- and agency-fixed effects. The full sample contains data from 1990 to 2013 (column 1). The GFC sample runs from 2009 to 2013 (column 2). Standard errors are clustered at both the agency-time and the sovereign level. ***,**,* indicate significance at the one-percent, five-percent or ten-percent level. P-values are displayed in brackets.

Appendix II, A12: Determinants of sovereign ratings (only control variables, all agencies pooled)

	(1)		(2)		(3)	
	Least squares				Ordered Probit	
	Full sample		GFC sample		Full sample	
GDP per capita (log)	2.048***	[0.000]	2.045***	[0.000]	1.101***	[0.000]
GDP growth	0.272***	[0.001]	0.143***	[0.006]	0.140***	[0.000]
GDP growth squared	-0.013**	[0.042]	-0.002	[0.477]	-0.007**	[0.043]
Inflation	-9.354***	[0.000]	-9.598***	[0.000]	-5.011***	[0.000]
Natural resources	-0.028***	[0.009]	-0.031**	[0.011]	-0.016**	[0.021]
Population (log)	0.723***	[0.000]	0.698***	[0.000]	0.416***	[0.000]
Change in government debt	-0.019	[0.238]	-0.021*	[0.095]	-0.006	[0.466]
Government debt	-0.010**	[0.020]	-0.002	[0.622]	-0.007**	[0.011]
Default (since 1970)	-1.387***	[0.000]	-1.612***	[0.000]	-0.659***	[0.000]
Default (last 5 years)	-1.847***	[0.000]	-1.537***	[0.000]	-0.983***	[0.000]
Trade openness	0.004*	[0.096]	0.006***	[0.009]	0.003*	[0.062]
Current account balance	0.077***	[0.004]	0.047	[0.114]	0.045***	[0.000]
External debt	-0.704	[0.172]	-0.581	[0.290]	-0.265	[0.367]
Rule of law	0.513***	[0.000]	0.639***	[0.000]	0.279***	[0.000]
Polity	0.013	[0.529]	0.026	[0.255]	0.017	[0.153]
Election	-0.101**	[0.021]	-0.122***	[0.007]	-0.069**	[0.020]
Years in office	0.017	[0.276]	0.026	[0.156]	0.015	[0.108]
Left government	-0.052	[0.776]	-0.030	[0.855]	-0.032	[0.797]
Absence of internal conflict	-0.037	[0.646]	-0.147**	[0.030]	-0.003	[0.952]
Absence of external conflict	0.235**	[0.017]	0.304***	[0.000]	0.108	[0.106]
Absence of military in politics	0.355***	[0.001]	0.227**	[0.024]	0.163**	[0.023]
Euro area	0.620***	[0.007]	0.778***	[0.000]	0.401**	[0.036]
Adj. R-Squared	0.86		0.86			
Number of observations	75,438		26,961		75,416	
Number of rated countries	107		104		107	

Notes: The dependent variable is a country's sovereign rating on a 21-point scale. All regressions include time- and agency-fixed effects. The full sample contains data from January 1990 to June 2013. The GFC sample runs from September 2008 to June 2013. Standard errors are clustered at both the agency-time and the sovereign level. ***,**,* indicate significance at the one-percent, five-percent or ten-percent level. P-values are displayed in brackets.

We run regressions that include only our control variables, i.e., exclude our variables of interest $x_{j,i,t}$. Since the previous literature has focused mainly on ratings from the big U.S.-based agencies, this exercise shows us whether our estimations with a larger set of agencies yield similar results. As can be seen from column 1 of the table above, the results are in line with former studies focusing on fewer agencies and shorter time periods. Both *GDP per capita* and *Inflation* show the expected signs and are statistically significant at the one-percent level. In line with Hill et al. (2010), the positive effect of *GDP growth* is diminishing as indicated by the significant negative squared term. *Natural resources* shows a negative coefficient which is significant at the one-percent level in line with the idea of a natural resource curse. *Population*

is positive and significant at the one-percent level; larger countries thus receive better ratings on average.

Turning to fiscal stability, we find that *change in government debt* and *government debt* both have the expected negative coefficients, but only the latter reaches statistical significance at conventional levels. Even conditional on the other variable, both *default* variables turn out to be negative and significant at the one-percent level. The fact that a country has defaulted since 1970 leads to a rating that is lower by 1.4 points on our 21-point scale. Countries that have defaulted over the last five years are further downgraded by 1.8 points on average. With regard to a country's external performance, *trade openness* and *current account balance* have a positive and significant effect on ratings as expected. *External debt* does not seem to further affect sovereign ratings conditional on the other factors.

Most political and institutional factors show the expected signs. The coefficient on *rule of law* is positive in agreement with Biglaiser and Staats (2012) and statistically significant at the one-percent level. In line with political-business-cycle considerations (Block and Vaaler 2004), rating levels in times of *elections* seem to be related to more uncertainty about future government policy, as indicated by the respective negative coefficient (significant at the five-percent level). Rating agencies also provide better ratings to countries characterized by *absence of external conflict* and *absence of military in politics*. All other political variables (*polity*, *years in office*, *left government*, and *absence of internal conflict*) do not reach statistical significance at conventional levels. As the "implicit bailout" guarantee would suggest, the coefficient on *euro area* is positive and significant at the one-percent level. Our results are similar when we restrict the observation period to the time after the onset of the GFC (column 2).

The objective country-specific controls alone explain 86 percent of the variation in sovereign ratings, as indicated by the R-squared value. Our model thus explains a large share of the variation in sovereign ratings from this broad set of agencies.

We also show in column 3 the results of an ordered probit model estimated with maximum likelihood estimation for the discrete 21-step rating at the end of a month. As expected, the results are qualitatively similar to the least squares estimation results in columns 1 and 2.

Appendix II, A13: Determinants of sovereign ratings (control variables of models in TableII. 3, column 1)

	(a)		(b)		(c)		(d)	
GDP per capita (log)	2.069***	[0.000]	2.048***	[0.000]	2.022***	[0.000]	1.969***	[0.000]
GDP growth	0.272***	[0.002]	0.272***	[0.001]	0.273***	[0.001]	0.294***	[0.004]
GDP growth squared	-0.013*	[0.056]	-0.013**	[0.042]	-0.013**	[0.040]	-0.014**	[0.038]
Inflation	-9.365***	[0.000]	-9.354***	[0.000]	-9.392***	[0.000]	-16.101***	[0.000]
Natural resources	-0.027**	[0.030]	-0.028***	[0.009]	-0.028***	[0.009]	-0.021*	[0.067]
Population (log)	0.744***	[0.000]	0.723***	[0.000]	0.708***	[0.000]	0.799***	[0.000]
Change in government debt	-0.018	[0.273]	-0.019	[0.238]	-0.019	[0.218]	-0.004	[0.883]
Government debt	-0.009**	[0.048]	-0.010**	[0.020]	-0.011**	[0.014]	-0.017***	[0.001]
Default (ever)	-1.391***	[0.000]	-1.387***	[0.000]	-1.371***	[0.000]	-1.407***	[0.000]
Default (last 5 years)	-1.847***	[0.000]	-1.847***	[0.000]	-1.854***	[0.000]	-2.400***	[0.000]
Trade openness	0.004*	[0.095]	0.004*	[0.096]	0.004*	[0.084]	0.004	[0.141]
Current account balance	0.075***	[0.000]	0.077***	[0.004]	0.078***	[0.003]	0.072**	[0.015]
External debt	-0.710	[0.202]	-0.704	[0.172]	-0.674	[0.186]	-1.041*	[0.054]
Rule of law	0.513***	[0.000]	0.513***	[0.000]	0.520***	[0.000]	0.412***	[0.002]
Polity	0.015	[0.479]	0.013	[0.529]	0.015	[0.453]	0.019	[0.364]
Election	-0.101*	[0.052]	-0.101**	[0.021]	-0.105**	[0.016]	-0.076	[0.149]
Years in office	0.018	[0.316]	0.017	[0.276]	0.017	[0.280]	0.022	[0.258]
Left government	-0.059	[0.780]	-0.052	[0.776]	-0.033	[0.857]	0.009	[0.968]
Absence of internal conflict	-0.016	[0.859]	-0.037	[0.646]	-0.047	[0.559]	0.069	[0.594]
Absence of external conflict	0.221**	[0.040]	0.235**	[0.017]	0.227**	[0.025]	0.154	[0.241]
Absence of military in politics	0.343***	[0.006]	0.355***	[0.001]	0.370***	[0.000]	0.510***	[0.000]
Euro area	0.595**	[0.030]	0.620***	[0.007]	0.690***	[0.003]	0.529*	[0.076]
Adjusted R-squared	0.86		0.86		0.87		0.87	
Observations	75,438		73,919		37,949		73,264	

Appendix II, A13 (continued): Determinants of sovereign ratings (control variables of models in Table II.3, column 1)

	(e)		(f)		(g)		(h)	
GDP per capita (log)	2.091***	[0.000]	2.053***	[0.000]	2.036***	[0.000]	2.087***	[0.000]
GDP growth	0.247***	[0.001]	0.274***	[0.001]	0.242***	[0.000]	0.231***	[0.000]
GDP growth squared	-0.012**	[0.046]	-0.013**	[0.039]	-0.009**	[0.019]	-0.009**	[0.033]
Inflation	-9.488***	[0.000]	-9.369***	[0.000]	-8.846***	[0.000]	-9.192***	[0.000]
Natural resources	-0.027**	[0.014]	-0.027**	[0.018]	-0.021**	[0.041]	-0.025**	[0.011]
Population (log)	0.734***	[0.000]	0.743***	[0.000]	0.719***	[0.000]	0.687***	[0.000]
Change in government debt	-0.019	[0.195]	-0.018	[0.244]	-0.023*	[0.070]	-0.030**	[0.013]
Government debt	-0.009**	[0.038]	-0.009**	[0.034]	-0.013***	[0.004]	-0.012***	[0.001]
Default (ever)	-1.411***	[0.000]	-1.394***	[0.000]	-1.506***	[0.000]	-1.525***	[0.000]
Default (last 5 years)	-1.852***	[0.000]	-1.837***	[0.000]	-1.929***	[0.000]	-1.914***	[0.000]
Trade openness	0.004*	[0.082]	0.004	[0.101]	0.009***	[0.004]	0.006***	[0.007]
Current account balance	0.072***	[0.007]	0.076***	[0.004]	0.033*	[0.051]	0.048***	[0.000]
External debt	-0.652	[0.216]	-0.717	[0.172]	-0.979**	[0.038]	-1.022**	[0.031]
Rule of law	0.513***	[0.000]	0.502***	[0.000]	0.370***	[0.000]	0.529***	[0.000]
Polity	0.017	[0.426]	0.012	[0.570]	0.022	[0.298]	0.032	[0.107]
Election	-0.101**	[0.018]	-0.103**	[0.020]	-0.155**	[0.014]	-0.101**	[0.029]
Years in office	0.018	[0.225]	0.018	[0.242]	0.021*	[0.079]	0.012	[0.332]
Left government	-0.051	[0.769]	-0.049	[0.788]	-0.059	[0.751]	-0.071	[0.649]
Absence of internal conflict	-0.010	[0.898]	-0.019	[0.811]	-0.045	[0.581]	-0.020	[0.790]
Absence of external conflict	0.234**	[0.013]	0.225**	[0.021]	0.245**	[0.012]	0.260***	[0.001]
Absence of military in politics	0.342***	[0.001]	0.340***	[0.001]	0.384***	[0.000]	0.317***	[0.001]
Euro area	0.622***	[0.007]	0.596**	[0.011]	1.200***	[0.002]	0.614**	[0.020]
Adjusted R-squared	0.86		0.85		0.89		0.87	
Observations	75,048		36,923		49,418		75,438	

Appendix II, A14: Adjusted R-squared and number of observations of OLS models in Table II.3

	(1) Full sample	(2) GFC sample
(a) Same country	0.86	0.87
	75438	26961
(b) Export interests	0.86	0.86
	73919	26461
(c) Bank exposure	0.87	0.87
	37949	22426
(d) Geopolitical alignment (UN)	0.86	0.86
	75048	26961
(e) US military interests (aid)	0.85	0.88
	36923	12405
(f) Common language	0.87	0.87
	75438	26961
(g) Cultural distance (language)	0.87	0.87
	75438	26961
(h) Cultural distance (ethno-racial)	0.86	0.86
	75438	26961

Note: The numbers of observations are identical in the corresponding ordered probit regressions.

Appendix II, A15: Home bias in sovereign ratings (OLS, pre- and post-GFC onset)

	(1)	(2)	(3)	(4)	(5)	(6)
				Controlled for *same country*		
	Full sample	Pre-GFC sample	GFC sample	Full sample	Pre-GFC sample	GFC sample
(a) Same country	0.950**	0.619*	1.704***			
	[0.018]	[0.079]	[0.001]			
(b) Export interests	0.017	0.004	0.030			
	[0.345]	[0.830]	[0.142]			
(c) Bank exposure	0.038*	0.026	0.045*			
	[0.054]	[0.152]	[0.070]			
(d) Geopolitical alignment (UN)	0.005	-0.007	0.013	0.000	-0.013*	0.008
	[0.518]	[0.265]	[0.183]	[0.964]	[0.077]	[0.467]
(e) US military interests (aid)	0.034*	0.021	0.116***	0.034*	0.021	0.116***
	[0.082]	[0.322]	[0.001]	[0.082]	[0.322]	[0.001]
(f) Common language	0.742***	0.699***	0.810***	0.695***	0.689***	0.653**
	[0.000]	[0.000]	[0.001]	[0.000]	[0.001]	[0.015]
(g) Cultural distance (language)	-0.021***	-0.017***	-0.027***	-0.021***	-0.017***	-0.025***
	[0.000]	[0.004]	[0.000]	[0.000]	[0.002]	[0.000]
(h) Cultural distance (ethno-racial)	0.001	0.000	0.003	0.002	0.001	0.005
	[0.718]	[0.900]	[0.472]	[0.499]	[0.757]	[0.172]

Notes: The dependent variable is a country's sovereign rating on a 21-point scale. Each cell refers to a separate regression. The table displays only the coefficients on the respective variable of interest of each regression. All regressions contain the control variables as specified in Appendix II, A12, time- and agency-fixed effects. The full sample contains data from January 1990 to June 2013 (columns 1 and 4). The pre-GFC sample runs from January 1990 to August 2008 (columns 2 and 5). The GFC sample runs from September 2008 to June 2013 (columns 3 and 6). Controlling for the *same country* dummy (columns 4-6) is not necessary in the case of *export interests* and *bank exposure* as these variables are not coded for the home country. Standard errors are clustered at both the agency-time and the sovereign level. ***,**,* indicate significance at the one-percent, five-percent or ten-percent level. P-values are displayed in brackets.

Appendix II, A16: Home bias in sovereign ratings
(all agencies pooled, additional control for same legal origin)

	(1)	(2)
	Full sample	GFC sample
(a) Same country	0.940**	1.520***
	[0.031]	[0.006]
(b) Export interests	0.017	0.027
	[0.390]	[0.222]
(c) Bank exposure	0.038*	0.050*
	[0.074]	[0.094]
(d) Geopolitical alignment (UN)	0.005	0.015***
	[0.375]	[0.002]
(e) US military interests (aid)	0.042*	0.138***
	[0.091]	[0.001]
(f) Common language	0.839***	0.957***
	[0.001]	[0.006]
(g) Cultural distance (language)	-0.021***	-0.030***
	[0.000]	[0.000]
(h) Cultural distance (ethno-racial)	0.001	0.006
	[0.630]	[0.124]

Notes: The dependent variable is a country's sovereign rating on a 21-point scale. Each cell refers to a separate regression. The table displays only the coefficients on the respective variable of interest of each regression. All regressions contain the control variables as specified in Appendix II, A12, time- and agency-fixed effects, and a dummy variable that takes a value of one if the home country and the rated country have the same legal origin. The full sample contains data from January 1990 to June 2013 (columns 1 and 2). The GFC sample runs from September 2008 to June 2013 (columns 3 and 4). Standard errors are clustered at both the agency-time and the sovereign level. ***,**,* indicate significance at the one-percent, five-percent or ten-percent level. P-values are displayed in brackets.

Appendix II, A17: Determinants of sovereign ratings (only control variables, by agency)

	CI	Dagong	DBRS	Feri	Fitch	JCR	Moody's	R&I	S&P
GDP per capita (log)	1.504***	1.980***	3.629***	1.843***	2.102***	1.658***	2.161***	2.113***	2.106***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
GDP growth	0.158	0.514***	0.630***	0.396***	0.189***	0.064	0.239***	0.420**	0.299***
	[0.164]	[0.000]	[0.000]	[0.005]	[0.008]	[0.492]	[0.001]	[0.024]	[0.000]
GDP growth squared	-0.018**	-0.015	-0.002	-0.027*	-0.007	-0.022	-0.008*	-0.037**	-0.018**
	[0.021]	[0.199]	[0.876]	[0.055]	[0.103]	[0.183]	[0.096]	[0.041]	[0.010]
Inflation	-14.319**	-12.909*	2.546	-6.759	-8.368***	-14.374***	-7.992***	-9.191	-11.360***
	[0.037]	[0.058]	[0.815]	[0.194]	[0.000]	[0.000]	[0.000]	[0.106]	[0.000]
Natural resources	-0.029	-0.042	0.107**	-0.061	-0.030**	0.086*	-0.030**	-0.056	-0.016
	[0.260]	[0.126]	[0.010]	[0.190]	[0.026]	[0.056]	[0.028]	[0.264]	[0.210]
Population (log)	0.651***	0.724***	1.389***	0.752***	0.718***	0.391*	0.706***	0.759***	0.681***
	[0.000]	[0.001]	[0.000]	[0.000]	[0.000]	[0.073]	[0.000]	[0.000]	[0.000]
Change in government debt	0.027	0.082	0.006	0.005	-0.037**	-0.025	-0.017	0.012	-0.035**
	[0.579]	[0.208]	[0.855]	[0.870]	[0.041]	[0.372]	[0.299]	[0.716]	[0.022]
Government debt	-0.02	-0.038***	-0.072***	-0.004	-0.010**	0.013*	-0.011**	-0.008	-0.012**
	[0.155]	[0.000]	[0.000]	[0.431]	[0.027]	[0.060]	[0.024]	[0.252]	[0.011]
Default (since 1970)	-1.157***	-0.684	-3.759***	-0.773	-1.564***	-1.534**	-1.814***	-1.682***	-1.376***
	[0.000]	[0.249]	[0.000]	[0.227]	[0.000]	[0.022]	[0.000]	[0.008]	[0.000]
Default (last 5 years)	-1.703	-4.852***	0.701	-0.286	-2.168***	-2.880***	-1.751***	-2.214***	-1.793***
	[0.524]	[0.000]	[0.229]	[0.687]	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]
Trade openness	0.006*	-0.004	0.006	-0.008**	0.005*	0.013*	0.005*	-0.002	0.007**
	[0.074]	[0.441]	[0.307]	[0.033]	[0.079]	[0.068]	[0.071]	[0.623]	[0.011]
Current account balance	0.056*	0.145***	-0.089**	0.248***	0.040**	-0.032	0.051***	0.103***	0.048**
	[0.064]	[0.001]	[0.027]	[0.000]	[0.033]	[0.464]	[0.006]	[0.005]	[0.010]

Appendix II, A17 (continued): Determinants of sovereign ratings (only control variables, by agency)

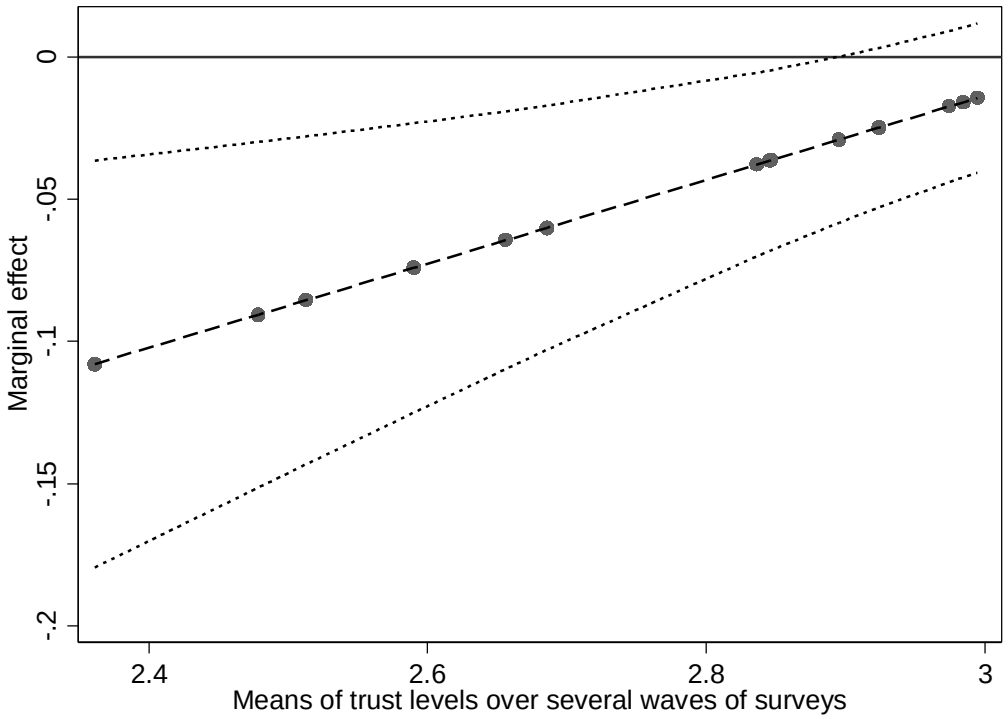
	CI	Dagong	DBRS	Feri	Fitch	JCR	Moody's	R&I	S&P
External debt	-1.639***	-2.015*	-10.539***	0.923	-1.088	-1.551*	-0.778	-0.567	-1.064*
	[0.002]	[0.077]	[0.000]	[0.469]	[0.104]	[0.069]	[0.193]	[0.334]	[0.073]
Rule of law	-0.426	0.124	-0.094	0.514**	0.564***	0.575*	0.480***	0.665***	0.545***
	[0.102]	[0.669]	[0.748]	[0.014]	[0.000]	[0.063]	[0.000]	[0.006]	[0.000]
Polity	-0.129***	-0.035	-0.421*	-0.082	-0.005	0.127	0.038	-0.03	0.043*
	[0.002]	[0.514]	[0.065]	[0.127]	[0.842]	[0.394]	[0.177]	[0.459]	[0.076]
Election	-0.057	-0.154	-0.055	-0.168**	-0.09	-0.008	-0.081	-0.161*	-0.136**
	[0.574]	[0.400]	[0.575]	[0.048]	[0.162]	[0.896]	[0.221]	[0.052]	[0.021]
Years in office	0.035	0.063*	-0.186***	0.073**	0.018	0.027	-0.001	0.022	0.015
	[0.252]	[0.088]	[0.000]	[0.020]	[0.327]	[0.325]	[0.948]	[0.429]	[0.271]
Left government	0.055	0.273	0.441*	-0.093	-0.109	0.13	-0.094	0.225	-0.024
	[0.889]	[0.532]	[0.056]	[0.699]	[0.610]	[0.541]	[0.685]	[0.509]	[0.903]
Absence of internal conflict	0.201	0.511*	0.439**	-0.042	-0.089	-0.350**	0.044	-0.117	-0.056
	[0.436]	[0.057]	[0.044]	[0.736]	[0.366]	[0.026]	[0.646]	[0.446]	[0.485]
Absence of external conflict	-0.073	-0.29	-0.016	0.118	0.279***	0.142	0.230**	0.292*	0.286***
	[0.746]	[0.338]	[0.903]	[0.440]	[0.005]	[0.360]	[0.032]	[0.095]	[0.005]
Absence of military in politics	1.010***	0.728**	0.966***	0.424*	0.340**	0.133	0.321**	0.312	0.303**
	[0.000]	[0.015]	[0.000]	[0.079]	[0.023]	[0.618]	[0.025]	[0.125]	[0.019]
Euro area	1.116	0.339	1.181***	0.696**	0.720**	0.820*	0.477	0.531*	0.589*
	[0.204]	[0.566]	[0.001]	[0.044]	[0.019]	[0.053]	[0.161]	[0.088]	[0.074]

Notes: The dependent variable is a country's sovereign rating on a 21-point scale. All regressions contain time- and agency-fixed effects. Data are from January 1990 to June 2013. Standard errors are clustered at both the sovereign and the time level. ***,**,* indicate significance at the one-percent, five-percent or ten-percent level. P-values are displayed in brackets.

Appendix II, A18: Adjusted R-squared and number of observations of OLS models in Table 6

		(1) CI CYP	(2) Dagong CHN	(3) DBRS CAN	(4) Feri DEU	(5) Fitch USA	(6) JCR JPN	(7) Moody's USA	(8) R&I JPN	(9) S&P USA	(10) CI KWT	(11) Fitch FRA
(a) Same country	Adj. R2	0.90	0.86	0.97	0.79	0.89	0.91	0.88	0.87	0.90	0.90	0.89
	Obs.	2,176	2,174	1,047	8,730	14,814	4,940	16,374	6,953	18,230	2,176	14,814
(b) Export interests	Adj. R2	0.91	0.86	0.97	0.79	0.90	0.91	0.88	0.87	0.90	0.90	0.90
	Obs.	2,176	2,174	1,047	8,730	14,814	4,940	16,374	6,953	18,230	2,176	14,814
(c) Bank exposure	Adj. R2	0.93	0.86	0.97	0.79	0.89	0.91	0.88	0.86	0.90	0.91	0.89
	Obs.	2,108	2,138	885	8,561	14,587	4,787	16,092	6,793	17,968	1,897	14,587
(d) Geopolitical alignment (UN)	Adj. R2	0.97	0.92	0.98	0.81	0.90	0.92	0.89	0.89	0.90	0.97	0.90
	Obs.	1,179	783	643	5,125	8,218	3,136	7,926	4,016	8,885	1,179	7,758
(e) US military interests (aid)	Adj. R2	0.90	0.92	0.97	0.79	0.90	0.91	0.88	0.87	0.90	0.91	0.89
	Obs.	2,176	783	1,047	8,730	14,814	4,940	16,374	6,953	18,230	2,176	14,814
(f) Common language	Adj. R2	0.90	0.87	0.97	0.80	0.89	0.91	0.88	0.87	0.90	0.90	0.89
	Obs.	2,176	2,174	1,047	8,698	14,724	4,940	16,229	6,953	18,107	2,176	14,724
(g) Cultural distance (language)	Adj. R2	0.97	0.92	1.00	0.81	0.85	0.90	0.84	0.88	0.85	0.97	0.89
	Obs.	1,179	783	423	5,203	11,216	2,672	12,041	3,942	13,666	1,179	10,236
(h) Cultural distance (ethno-racial)	Adj. R2	0.97	0.92	1.00	0.81	0.90	0.90	0.88	0.88	0.90	0.97	0.89
	Obs.	1,179	783	423	5,203	14,814	2,672	16,374	3,942	18,230	1,179	10,236

Appendix II, A19: Marginal effect of cultural distance language at different levels of bilateral trust (Feri only)



Note: Dotted lines show ninety-percent confidence interval.

Appendix II, A20: Home bias in sovereign ratings
(all agencies pooled, Heckman selection model)

	(1)		(2)		(3)		(4)	
(a) Same country	0.979**	[0.022]	0.961**	[0.023]	0.942**	[0.025]	0.936**	[0.025]
Rho	0.196		0.145		0.109		0.082	
Wald test Chi2 (p-value)	16.880	0.000	7.091	0.008	2.053	0.152	1.070	0.301
(b) Export interests	0.009	[0.624]	0.010	[0.559]	0.012	[0.475]	0.013	[0.451]
Rho	0.209		0.000		0.000		0.000	
Wald test Chi2 (p-value)	18.365	0.000	7.071	0.008	2.042	0.153	1.045	0.000
(c) Bank exposure	0.038*	[0.069]	0.038*	[0.070]	0.039*	[0.067]	0.039*	[0.068]
Rho	0.126		0.000		0.000		0.000	
Wald test Chi2 (p-value)	3.659	0.056	1.829	0.176	2.128	0.145	1.665	0.000
(d) Geopolitical alignment (UN)	0.004	[0.421]	0.004	[0.407]	0.004	[0.418]	0.004	[0.408]
Rho	0.205		0.000		0.000		0.000	
Wald test Chi2 (p-value)	19.166	0.000	7.968	0.005	2.578	0.108	1.447	0.000
(e) US military interests (aid)	0.043	[0.107]	0.043	[0.109]	0.038	[0.147]	0.038	[0.146]
Rho	0.341		0.000		0.000		0.000	
Wald test Chi2 (p-value)	12.474	0.000	12.378	0.000	6.641	0.010	6.908	0.000
(f) Common language	0.704***	[0.001]	0.712***	[0.001]	0.718***	[0.001]	0.722***	[0.001]
Rho	0.181		0.000		0.000		0.000	
Wald test Chi2 (p-value)	15.823	0.000	5.974	0.015	1.636	0.201	0.805	0.000
(g) Cultural distance (language)	-0.021***	[0.000]	-0.021***	[0.000]	-0.021***	[0.000]	-0.021***	[0.000]
Rho	0.180		0.000		0.000		0.000	
Wald test Chi2 (p-value)	16.478	0.000	6.389	0.011	1.692	0.193	0.860	0.000
(h) Cultural distance (ethno-racial)	0.001	[0.586]	0.001	[0.594]	0.001	[0.580]	0.001	[0.585]
Rho	0.211		0.000		0.000		0.000	
Wald test Chi2 (p-value)	21.403	0.000	9.598	0.002	2.574	0.109	1.416	0.000
Exclusion variable	*None*		*Number of rated countries in the previous period (by agency)*		*Number of "Big Three" rating a particular country in the previous period (by sovereign)*		*Both*	

Notes: The dependent variable of the second stage is a country's sovereign rating on a 21-point scale. The dependent variable of the first stage is a dummy variable that takes a value of one if the sovereign receives a rating from a particular agency. Each cell refers to a separate regression. The table displays only the coefficients on the respective variable of interest of each regression of the second stage. All regressions include control variables, time- and agency-fixed effects. The last row indicates the respective exclusion variables that are only included in the first-stage regression. The full sample contains data from February 1990 to June 2013. Standard errors are clustered at sovereign level. ***,**,* indicate significance at the one-percent, five-percent or ten-percent level. P-values are displayed in brackets.

Appendix II, A21: Unsolicited ratings and home bias in sovereign ratings (S&P only)

	I. Variable of interest		II. Unsolicited rating		III. Interaction		Test for significance of I. + III.
(a) Same country	0.738	[0.137]	0.054	[0.896]			
(b) Export interests	0.038	[0.176]	-0.237	[0.662]	0.142	[0.228]	(0.135)
(c) Bank exposure	-0.005	[0.964]	-0.264	[0.609]	0.169	[0.140]	(0.001)
(d) Geopolitical alignment (UN)	-0.015	[0.296]	-1.618	[0.111]	0.035**	[0.033]	(0.108)
(e) US military interests (aid)	0.034	[0.272]	0.098	[0.875]	-0.243*	[0.058]	(0.100)
(f) Common language	0.738**	[0.014]	-0.217	[0.687]	0.384	[0.530]	(0.034)
(g) Cultural distance (language)	-0.027**	[0.027]	0.233	[0.852]	-0.004	[0.806]	(0.023)
(h) Cultural distance (ethno-racial)	0.011	[0.372]	2.692**	[0.018]	-0.037**	[0.038]	(0.116)

Notes: The dependent variable is a country's sovereign rating on a 21-point scale. Each row refers to a separate regression. All regressions contain the control variables as specified in Appendix II, A12 and time-fixed effects. The full sample contains data from January 1990 to June 2013. Standard errors are clustered at both the sovereign and the time level. ***,**,* indicate significance at the one-percent, five-percent or ten-percent level. P-values are displayed in brackets. The final column displays the p-values of a Wald test for significance of the sum of the respective variable of interest and its interaction with the dummy for unsolicited ratings.

Interpretation: In most cases, we find no statistically significant relationship between the fact that a rating is unsolicited and the size of the bias. There are three cases where we find a significant interaction between a variable of interest and the dummy for unsolicited ratings. In all three cases, however, the main effect (column I., for solicited ratings) and the marginal effect (column I. + III., for unsolicited ratings) of the variables *geopolitical alignment (UN)*, *US military interests (aid)* and *cultural distance (ethno-racial)* do not reach statistical significance at conventional levels.

Appendix II, A22: Detailed sources of overview tables

Capital Intelligence (2012). *Transparency Report June 2012.* Available at: http://www.ciratings.com/images/uploads/Documents/Transparency_Report_June_2012.pdf (accessed 31 May 2013).

Capital Intelligence (2013a). *Sovereign Ratings*. Available at: http://www.ciratings.com/page/rating-methodologies/sovereign-ratings (accessed 25 March 2013).

Capital Intelligence (2013b). *Rating Process*. Available at: http://www.ciratings.com/page/policies-procedures/rating-process (accessed 25 March 2013).

Capital Intelligence (2013c). *About Us*. Available at: http://www.ciratings.com/page/about-us (accessed 23 May 2013).

Dagong Global Credit Rating (2013a). *Code of Conduct*. Available at: http://en.dagongcredit.com/ratingSystem/contents.html (accessed 21 March 2013).

Dagong Global Credit Rating (2013b). *Building a New Sovereign Rating Standard*. Available at: http://en.dagongcredit.com/content/details58_6634.html (accessed 21 March 2013).

Dagong Global Credit Rating (2013c). *About Us*. Available at: http://en.dagongcredit.com/about/index.html (accessed 31 May 2013).

DBRS (2011). *Methodology: Rating Sovereign Governments*. Available at: http://www.dbrs.com/research/239680/rating-sovereign-governments.pdf (accessed 21 March 2013).

DBRS (2013a). *Annual Certification - Exhibit 2 April 1 2013*. Available at: http://www.dbrs.com/research/215035/exhibit-2.pdf (accessed 4 April 2013).

DBRS (2013b). *Rating and Other Information: The Rating Process – Corporate and Structured Finance*. Available at: http://www.dbrs.com/research/236737/the-rating-process-corporate-and-structured-finance.pdf (accessed 4 April 2013).

DBRS (2013c). *Rating and Other Processes: Ratings Appeal Policy.* Available at: http://www.dbrs.com/research/236746/rating-appeal.pdf (accessed 4 April 2013).

DBRS (2013d). *Biographies of the Board of Directors*. Available at: http://www.dbrs.com/research/256664/director-biographies.pdf (accessed 31 May 2013).

DBRS (n.d.). *Rating and Other Processes: Rating Committee – Corporate and Structured Finance*. Available at: http://www.dbrs.com/research/236744/ (accessed 21 March 2013).

Feri EuroRating Services (2009). *Richtlinie für die Erstellung von Ratings für den Bereich Credit Rating*. Available at: http://www.feri.de/files/documents/fer/Downloads/LEGAL/Policies/Feras_Richtlinie_%20Erstellung_von_Ratings.pdf (accessed 25 March 2013).

Feri EuroRating Services (2013a). *Feri Country Dossier: Economic Forecast – Canada*. Available at: http://frr.feri.de/files/documents/fer/beispielreports/bsp_countryrating.pdf (accessed 4 April 2013).

Feri EuroRating Services (2013b). *Portrait*. Available at: http://www.feri.de/de/das-unternehmen/portrait/ (accessed 23 May 2013).

Fimalac (2013). *Le Groupe Fimalac: Organigramme*. Available at: http://www.fimalac.com/organigramme.html (accessed 31 May 2013).

Financial Times Deutschland (2006). *Harald-Quandt-Erben steigen bei Feri aus*. Available at: http://www.ftd.de/karriere/management/harald-quandt-erben-steigen-bei-feri-aus/114333.html (accessed 31 May 2013).

Fitch IBCA (2011). *The Rating Process – Special Report*. Available at: http://www.fitchratings.com/web_content/nrsro/nav/NRSRO_Exhibit-2.pdf (accessed 25 March 2013).

Fitch IBCA (n.d.). *Sovereign Ratings: Rating Methodology*. Available at: http://rru.worldbank.org/Documents/Toolkits/Highways/pdf/extract/E24.pdf (accessed 14 March 2013).

Fitch (2013). *About Us*. Available at: http://www.fitchratings.com/web/en/dynamic/about-us/about-us.jsp (accessed 23 May 2013).

Gulf Master International (n.d.). *Gulf Master International General Trading and Contracting Company*. Available at: http://www.gulf-master.com/new/client/cms-inner-page.asp?id=12 (accessed 31 July 2013).

Japan Credit Rating Agency (2013). *About JCR*. Available at: http://www.jcr.co.jp/english/company/index.html (accessed 23 May 2013).

Japan Credit Rating Agency (2012a). *JCR's Credit Rating Process*. Available at: http://www.jcr.co.jp/english/rat_poli/pdf/top_cont_2_3.pdf?PHPSESSID=bef10584b270e3887d16c7915596b20c (accessed 25 March 2013).

Japan Credit Rating Agency (2012b). *Types of Credit Ratings and Definitions of Rating Symbols*. Available at: http://www.jcr.co.jp/english/rat_poli/pdf/top_cont_6.pdf (accessed 25 March 2013).

Japan Credit Rating Agency (2013). *Sovereign and Public Sector Entities*. Available at: http://www.jcr.co.jp/english/coc/coc13_2.pdf?PHPSESSID=eaefd9b1c2100e6157fa7def4677fb08 (accessed 4 April 2013).

Japan Credit Rating Agency (n.d.). *The Rating Policy*. Available at : http://www.jcr.co.jp/english/rat_poli/pdf/top_cont_1.pdf?PHPSESSID=4a946f4a063936f187ddc470d3fb8673 (accessed 25 March 2013).

Marketwire (2006). *FIMALAC Enters Into Agreement to Sell Minority Share in Fitch Group to Hearst*. Available at: http://www.marketwired.com/press-release/fimalac-enters-into-agreement-to-sell-minority-share-in-fitch-group-to-hearst-686571.htm (accessed 31 May 2013).

MLP (2013). *Die Aktionärsstruktur von MLP*. Available at: http://www.mlp-ag.de/#/investor-relations/mlp-aktie/aktionaersstruktur (accessed 31 May 2013).

Moody's (2013a). *Rating Symbols and Definitions*. Available at: http://de.scribd.com/doc/125270001/Moody-s-Rating-Symbols-and-Definitions-January-2013 (accessed 25 March 2013).

Moody's (2013b). *How to Get Rated*. Available at: http://www.moodys.com/Pages/amr002001.aspx (accessed 21 March 2013).

Moody's (2013c). *Moody's Investor Relations*. Available at: http://ir.moodys.com/index.cfm (accessed 31 March 2013).

Morningstar (2013). *McGraw Hill Financial Inc*. Available at : http://investors.morningstar.com/ownership/shareholders-overview.html?t=MHFI&region=USA (accessed 31 May 2013).

Nasdaq (2013). *Moody's Corporation Institutional Ownership*. Available at: http://www.nasdaq.com/symbol/mco/institutional-holdings (accessed 31 March 2013).

Rating and Investment Information (2012a). *R&I's Analytical Approach to Sovereigns*. Available at: http://www.ri.co.jp/eng/body/cfp/topics_methodology/2012/03/topics_methodology_20120316_807537779_01.pdf (accessed 14 March 2013).

Rating and Investment Information (2012b). *金融商品取引法第66条の39の規定に基づく説明書類です*. Available at: http://www.r-i.co.jp/jpn/regulatory_affair/fiea/ (accessed 6 June 2013).

Rating and Investment Information (2013a). *R&I's Analytical Approach to Sovereigns*. Available at: http://www.r-http://www.r-i.co.jp/eng/company/profile.html (accessed 14 March 2013).

Rating and Investment Information (2013b). *Company Information*. Available at: http://www.r-i.co.jp/eng/company/history.html (accessed 23 May 2013).

Rating and Investment Information (n.d.). *Corporate History*. Available at: http://www.r-i.co.jp/eng/cfp/about/process.pdf (accessed 23 May 2013).

Seeking Alpha (2010). *SEC Denies Market Entry to China's Dagong; Dagong Unlikely to Go Away Quietly*. Available at: http://seekingalpha.com/article/227248-sec-denies-market-entry-to-china-s-dagong-dagong-unlikely-to-go-away-quietly (accessed 31 May 2013).

Standard & Poor's (2012). *How We Rate Sovereigns*. Available at: http://www.standardandpoors.com/spf/ratings/How_We_Rate_Sovereigns_3_13_12.pdf (accessed 21 March 2013).

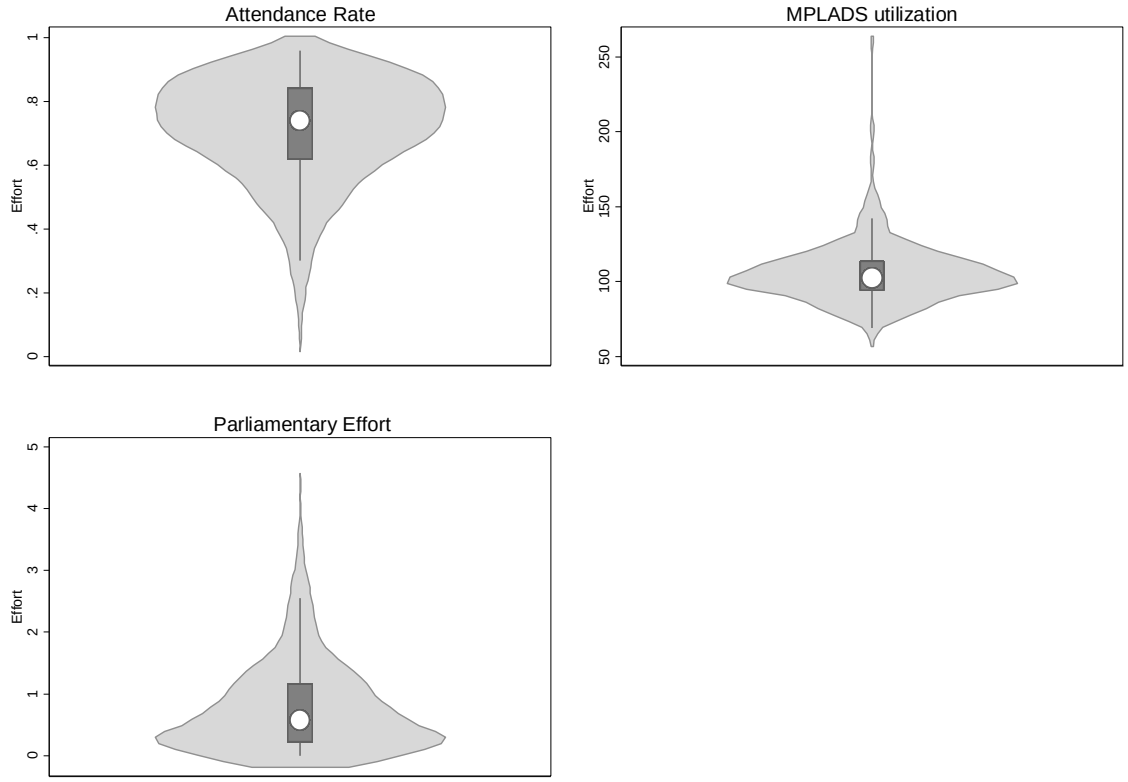
Standard & Poor's (2013a). *Ratings*. Available at: http://www.standardandpoors.com/ratings/en/eu/ (accessed 25 March 2013).

Standard & Poor's (2013b). *Sovereigns Rating List*. Available at: https://www.standardandpoors.com/ratings/sovereigns/ratings-list/en/us/?subSectorCode=39&sectorId=1221186707758&subSectorId=1221187348494 (accessed 23 May 2013).

TAZ (2011). *Guan Jianzhong von Ratingagentur Dagong: "Ideologie ist kein Maßstab"*. Available at: http://www.taz.de/Guan-Jianzhong-von-Ratingagentur-Dagong/!75076/ (accessed 23 May 2013).

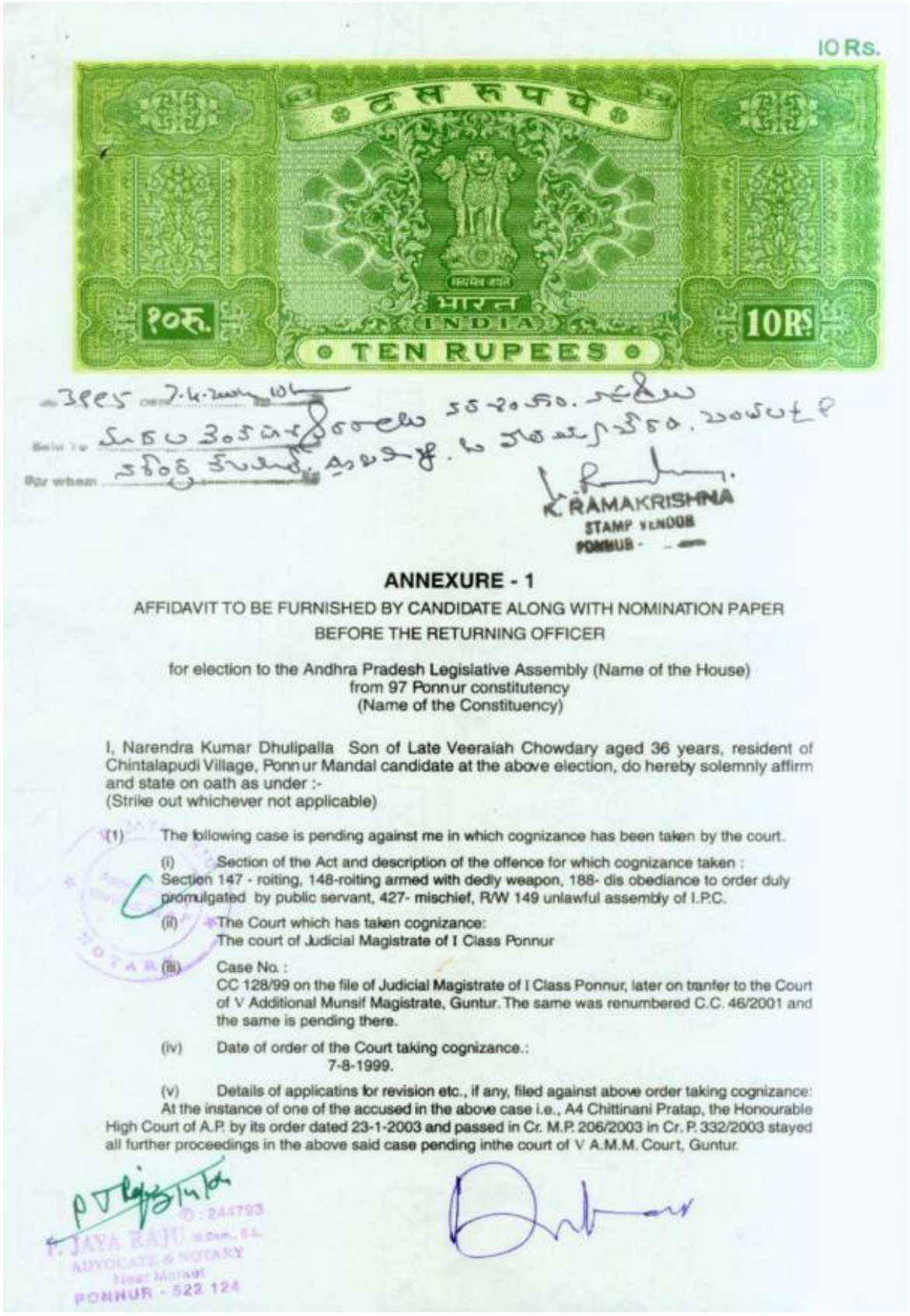
Appendix IIIB

Appendix IIIB, Figure 1: Violinplots of dependent variables



Notes: Violin plots are a modification of box plots that add plots of the estimated kernel density to the summary statistics displayed by box plots. The white dot indicates the median value, the box comprises the 25th to 75th percentiles. Points beyond the upper and lower adjacent values indicate potential outliers.

Appendix IIIB, Figure 2: Example of the affidavits that were used for coding the criminal variable



10 Rs.

दस रुपये

भारत
INDIA

TEN RUPEES

10RS

K. RAMAKRISHNA
STAMP VENDOR
PONNUR

ANNEXURE - 1

AFFIDAVIT TO BE FURNISHED BY CANDIDATE ALONG WITH NOMINATION PAPER BEFORE THE RETURNING OFFICER

for election to the Andhra Pradesh Legislative Assembly (Name of the House)
from 97 Ponnur constitutency
(Name of the Constituency)

I, Narendra Kumar Dhulipalla Son of Late Veeraiah Chowdary aged 36 years, resident of Chintalapudi Village, Ponnur Mandal candidate at the above election, do hereby solemnly affirm and state on oath as under :-
(Strike out whichever not applicable)

(1) The following case is pending against me in which cognizance has been taken by the court.

(i) Section of the Act and description of the offence for which cognizance taken :
Section 147 - roiting, 148-roiting armed with dedly weapon, 188- dis obediance to order duly promulgated by public servant, 427- mischief, R/W 149 unlawful assembly of I.P.C.

(ii) The Court which has taken cognizance:
The court of Judicial Magistrate of I Class Ponnur

(iii) Case No. :
CC 128/99 on the file of Judicial Magistrate of I Class Ponnur, later on tranfer to the Court of V Additional Munsif Magistrate, Guntur. The same was renumbered C.C. 46/2001 and the same is pending there.

(iv) Date of order of the Court taking cognizance.:
7-8-1999.

(v) Details of applicatins for revision etc., if any, filed against above order taking cognizance:
At the instance of one of the accused in the above case i.e., A4 Chittinani Pratap, the Honourable High Court of A.P. by its order dated 23-1-2003 and passed in Cr. M.P. 206/2003 in Cr. P. 332/2003 stayed all further proceedings in the above said case pending inthe court of V A.M.M. Court, Guntur.

P. JAYA RAJU
ADVOCATE & NOTARY
Near Market
PONNUR - 522 124

Notes: Main source was http://eci.nic.in/archive/GE2004/States/index_fs.htm, an alternative source which does not contain all constituencies is http://myneta.info/loksabha2004/.

Appendix IIIB, Table 1: Frequency of Crimes

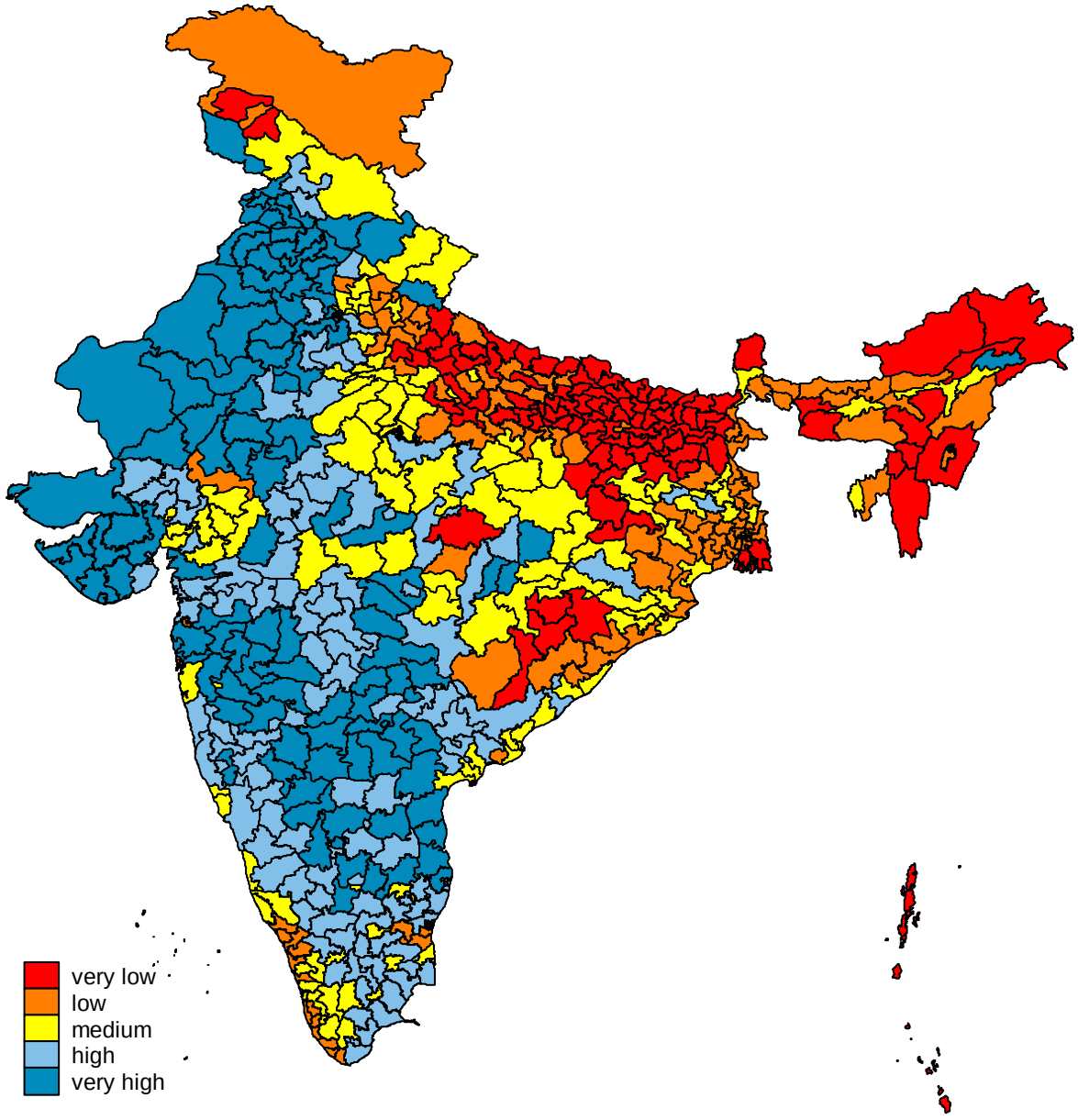
Number of Crimes	Frequency	Percentage	Specification 1	Specification 2	Specification 3
0	336	[76.54%]	Non-Criminals	Non-Criminals	Non-Criminals
1	54	[12.30%]	Criminal(a)		Excluded
2	20	[4.56%]		Criminal(b)	Criminal(b)
3	8	[1.82%]			
4	7	[1.59%]			
5	3	[0.68%]			
8	1	[0.23%]			
9	1	[0.23%]			
13	3	[0.68%]			
18	1	[0.23%]			

Notes: Specification 1 is the main specification, used for example in Table 3, column 1-3. Specification 2 is used in all specifications using *Criminal(b)*, for example Table 3, column 4-6. The one exception is the last row in Table 5.2, where Specification 3 is used as a robustness check.

Appendix IIIB, Table 2: Criminals by state

State\ Status	Normal		Criminal(a)			Normal		Criminal(a)	
Andaman Nicobar	1	[100.0%]	0	[0.0%]	Maharashtra	21	[53.8%]	18	[46.2%]
Andhra Pradesh	29	[90.6%]	3	[9.4%]	Manipur	2	[100.0%]	0	[0.0%]
Arunachal Pradesh	2	[100.0%]	0	[0.0%]	Meghalaya	1	[100.0%]	0	[0.0%]
Assam	14	[100.0%]	0	[0.0%]	Mizoram	1	[100.0%]	0	[0.0%]
Bihar	19	[61.3%]	12	[38.7%]	NCT of Delhi	3	[60.0%]	2	[40.0%]
Chhattisgarh	6	[75.0%]	2	[25.0%]	Nagaland	1	[100.0%]	0	[0.0%]
Dadra & Nagar Haveli	1	[100.0%]	0	[0.0%]	Orrisa	16	[84.2%]	3	[15.8%]
Daman & Diu	0	[0.0%]	1	[100.0%]	Pondicherry	1	[100.0%]	0	[0.0%]
Goa	1	[100.0%]	0	[0.0%]	Punjab	7	[63.6%]	4	[36.4%]
Gujarat	17	[73.9%]	6	[26.1%]	Rajasthan	20	[87.0%]	3	[13.0%]
Haryana	7	[87.5%]	1	[12.5%]	Sikkim	1	[100.0%]	0	[0.0%]
Himachal Pradesh	3	[100.0%]	0	[0.0%]	Tamil Nadu	28	[75.7%]	9	[24.3%]
Jammu & Kashmir	4	[100.0%]	0	[0.0%]	Tripura	2	[100.0%]	0	[0.0%]
Jharkhand	4	[44.4%]	5	[55.6%]	Uttar Pradesh	46	[74.2%]	16	[25.8%]
Karnataka	15	[75.0%]	5	[25.0%]	Uttaranchal	3	[100.0%]	0	[0.0%]
Kerela	12	[63.2%]	7	[36.8%]	West Bengal	34	[94.4%]	2	[5.6%]
Madhya Pradesh	13	[72.2%]	5	[27.8%]	Total	335	[76.3%]	104	[23.7%]

Appendix IIIB, Figure 3: Constituency-level approximation of economic development based on nighttime light intensity using satellite data.



Notes: Created using average visible, stable light and cloud free from the F16 satellite for 2004. The original description states that "The cleaned up (file) contains the lights from cities, towns, and other sites with persistent lighting, including gas flares. Ephemeral events, such as fires have been discarded. Then the background noise was identified and replaced with values of zero. Data values range from 1-63. Areas with zero cloud-free observations are represented by the value 255." More information can be found at http://ngdc.noaa.gov/eog/gcv4_readme.txt. We use the tif-image-file from the National Geophysical Data Center and merged it in ArcGIS with constituency boundaries that were shared by Aidt et al. (2015). We then calculated the sum of lights using zonal statistics within the constituencies to proxy for economic development.

Appendix IIIB, Table 3: Relation between dropping out of sample, dependent variable and variable of interest

Dependent variable	Criminal Winner(a)		MPLADS	
MP change from MP data	0.005	[0.048]	1.843	[2.508]
Bharatiya Janata Party	-0.087	[0.064]	1.018	[3.307]
Communist Party of India (Marxist)	-0.032	[0.099]	8.582*	[5.102]
Indian National Congress	-0.078	[0.056]	-2.720	[2.906]
Rashtriya Janata Dal	0.137	[0.127]	-2.731	[6.573]
Samajwadi Party	0.006	[0.095]	-3.243	[4.904]
Reserved seat (SC or ST)	-0.039	[0.046]	-1.774	[2.375]
Voter turnout (2004)	-0.216	[0.246]	-18.810	[12.743]
No of voters	-0.005	[0.028]	-0.798	[1.446]
Party Stronghold (3time winner)	-0.004	[0.002]	0.285**	[0.125]
Party Stronghold	-0.028	[0.060]	4.401	[3.112]
Margin (2004)	0.006	[0.201]	-12.334	[10.428]
Economic Development	-0.003*	[0.002]	0.039	[0.089]
Candidate Age (at election)	-0.042*	[0.025]	1.243	[1.273]
Formal Education in 3 steps	-0.009	[0.021]	-2.213**	[1.110]
Experience in parliament	0.090	[0.069]	-1.722	[3.574]
Gender	0.009	[0.018]	0.193	[0.907]
Log of net assets	-0.039	[0.046]	-1.774	[2.375]
Adjusted R-Squared	0.06		0.03	
Number of constituencies	540		540	
SE's clustered at	State level		State level	

Notes: Analyzes whether there is a relation between *Criminal(a)* and MP's dropping out of parliament, and between the dependent variable MPLADS utilization and MP's dropping out of parliament. Standard errors are clustered at the state level. If *Criminal(a)* would be significantly related to the change, this could bias our results. If it would be significantly related to our dependent variables, it would be an omitted variable bias problem. We are only able to capture the value of the dependent variable for those constituencies with a change during the term. *Attendance rates* and *Parliamentary activity* are not provided for those constituencies with a change in MP. We can see in both regressions that there is no significant relationship; hence this does not affect our results.

Appendix IIIB, Table 4: Baseline results

	Attendance rate		Parliamentary activity		MPLADS utilization	
	(1)		(2)		(3)	
Bharatiya Janata Party	-0.003	[0.013]	-0.097	[0.116]	-2.033	[2.010]
Communist Party of India	0.063	[0.039]	-0.373**	[0.156]	5.680	[4.190]
Indian National Congress	0.054***	[0.014]	-0.128	[0.104]	-3.784*	[2.110]
Rashtriya Janata Dal	0.026	[0.018]	0.288**	[0.123]	-4.187	[3.545]
Samajwadi Party	0.074**	[0.028]	0.160*	[0.087]	-4.052	[2.733]
PC reserved for minority	0.058***	[0.013]	-0.104	[0.100]	-2.100	[2.388]
Party stronghold (3time winner)	0.031	[0.031]	0.024	[0.154]	0.929	[2.552]
Winning margin (2004)	-0.175*	[0.094]	-0.539	[0.333]	-5.665	[6.516]
Voter turnout (2004)	-0.232***	[0.076]	-0.381	[0.634]	-15.162	[10.951]
Economic development	-0.011	[0.015]	0.101*	[0.057]	0.479	[1.430]
Literacy rate	0.002***	[0.001]	0.003	[0.003]	0.143	[0.114]
Candidate age (at election)	0.003***	[0.001]	0.000	[0.003]	0.008	[0.109]
Formal education in 3 steps	0.024***	[0.007]	0.046	[0.070]	0.289	[1.663]
Experience in parliament	-0.013	[0.012]	0.018	[0.040]	-1.250	[1.119]
Gender	-0.014	[0.032]	0.207*	[0.105]	-0.305	[3.962]
Net assets (log)	-0.019**	[0.008]	-0.001	[0.032]	-0.362	[0.457]
R-Squared	0.29		0.11		0.08	
Number of MPs	394		394		439	
State Dummies	Yes		Yes		Yes	

Notes: Dependent variable as specified above over the full legislative period 2004-2009, MPLADS 2005-2008. Standard errors are clustered at the party level. *** (**, *) indicates significance at the 1 (5, 10) percent level respectively.

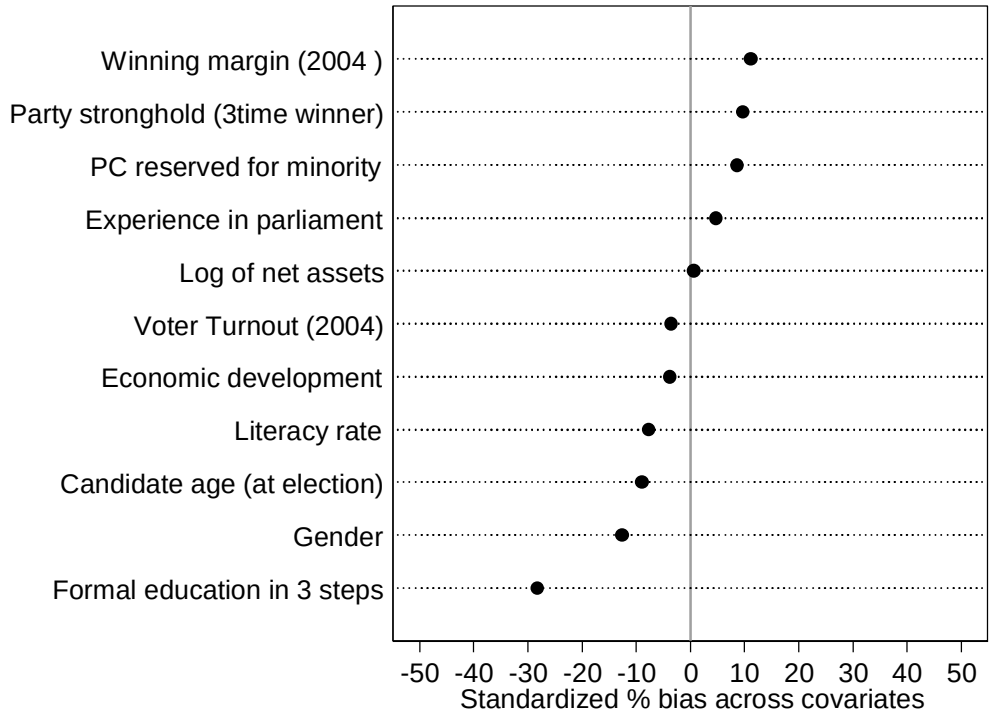
Descriptive statistics for the matching specifications:

Appendix IIIB, Table 5: Descriptive statistics for treated and control group

	Mean			t-test	
Variable	Treated	Control	%bias	t	p>t
PC reserved for minority	0.25	0.19	14.7	1.05	0.297
Party stronghold (3time winner)	0.17	0.13	9.7	0.74	0.458
Winning margin (2004)	0.10	0.09	12.9	1.06	0.288
Voter turnout (2004)	0.57	0.57	-5	-0.38	0.704
GDP (sum of night lights)	9.70	9.71	-1.6	-0.11	0.914
Literacy rate	54.79	56.23	-11.5	-0.73	0.464
Candidate age (at election)	49.85	51.23	-13.4	-0.96	0.34
Formal education in 3 steps	1.48	1.70	-27.9	-2.05	0.041
Experience in parliament	0.56	0.58	-2.4	-0.18	0.858
Gender	0.96	0.98	-7.3	-0.67	0.503
Log of net assets	16.05	16.12	-6.8	-0.61	0.541

Notes: Relates to Table 6. T-test is a simple t-test of differences in the mean. Outcome variable is *attendance rate.*

Appendix IIIB, Figure 4: Matching balance



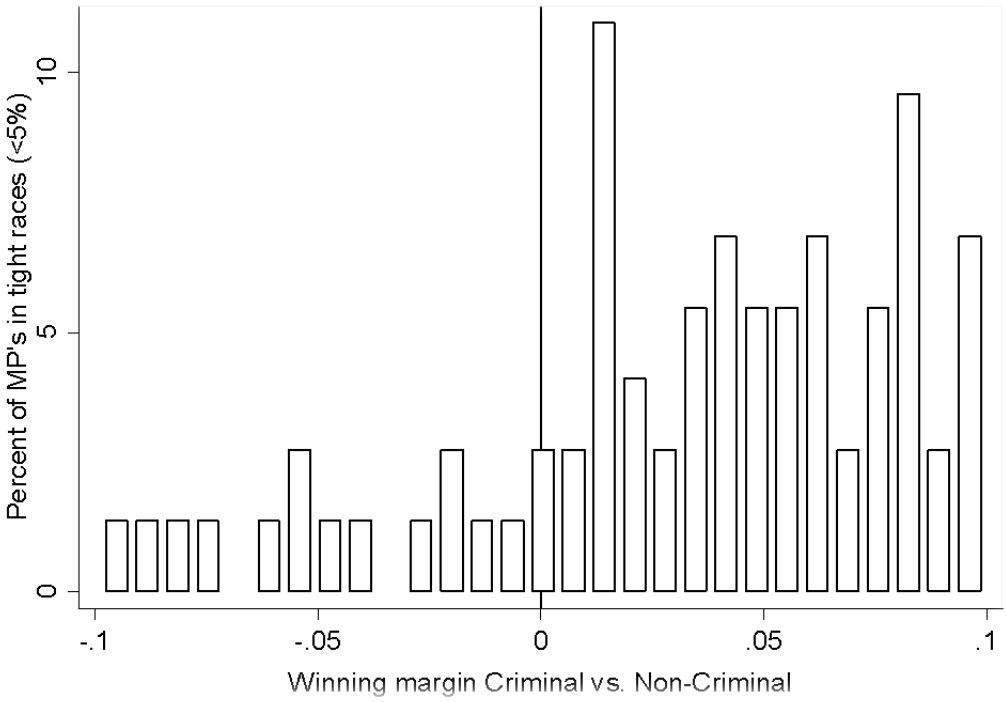
Notes: Relates to Table 6. Graphical depiction of matching balance. Results remain qualitatively unchanged when matching exactly on education.

Appendix IIIB, Table 6: Selection equations for treatment effect regressions

Dependent variable in second stage	Attendance rate		Parliamentary activity		MPLADS	
Dependent variable in selection equation	Criminal(a)		Criminal(a)		Criminal(a)	
Criminal(a)	**-0.578****	**[0.229]**	**-0.559****	**[0.239]**	**-0.513**	**[0.349]**
Bharatiya Janata Party	0.081	[0.384]	0.031	[0.404]	0.095	[0.439]
Communist Party of India	-0.354**	[0.155]	-0.380**	[0.167]	-0.472	[0.311]
Indian National Congress	0.359	[0.430]	0.359	[0.418]	0.568***	[0.209]
Rashtriya Janata Dal	0.147	[0.186]	-0.021	[0.142]	0.017	[0.154]
Samajwadi Party	0.064	[0.218]	0.081	[0.236]	0.014	[0.166]
PC reserved for minority	-1.561	[1.506]	-1.749	[1.642]	-1.552	[1.027]
Voter Turnout (2004)	-0.061	[0.114]	-0.008	[0.106]	0.009	[0.117]
GDP (log sum of night lights)	-0.018*	[0.009]	-0.018**	[0.009]	-0.023*	[0.014]
Literacy rate	-0.004	[0.302]	0.003	[0.272]	-0.091	[0.250]
Party Stronghold (3time winner)	-0.083	[0.907]	0.103	[0.793]	0.347	[0.729]
Margin (2004)	-0.014***	[0.005]	-0.012**	[0.005]	-0.012	[0.009]
Candidate Age (at election)	-0.142***	[0.048]	-0.153***	[0.053]	-0.186*	[0.097]
Formal Education in 3 steps	-0.102	[0.062]	-0.088	[0.058]	-0.094	[0.067]
Experience in parliament	0.579	[0.352]	0.523	[0.336]	0.294	[0.299]
Gender	0.048	[0.095]	-0.010	[0.027]	0.012	[0.051]
Number of other contesting candidates with charges	0.168**	[0.083]	0.207**	[0.100]	0.152*	[0.092]
State Dummies	Yes		Yes		Yes	
SE's clustered at	State level		State level		State level	
Number of MPs	394		394		439	
Lamda	0.09		0.12		4.28	
Rho	0.57		0.16		0.22	
Prob>Chi2	0.0744		0.1183		0.004	

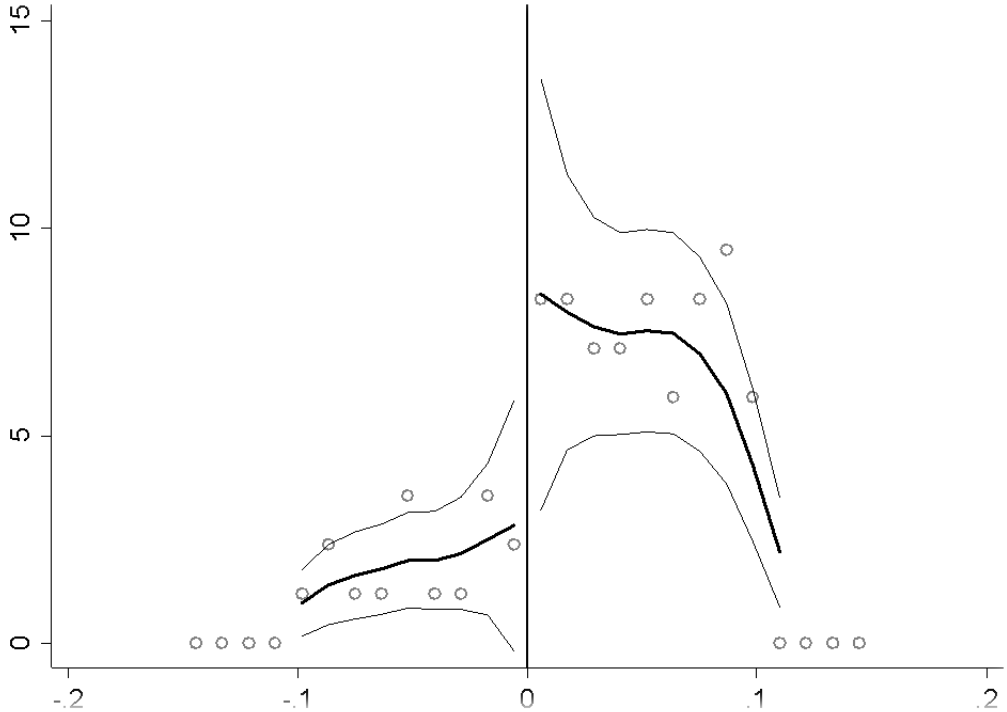
Notes: Dependent variable as specified above over the full legislative period 2004-2009, MPLADS 2005-2008. Second stage results for Criminal(a) see Table 6. Standard errors are clustered at the party level. *** (**, *) indicates significance at the 1 (5, 10) percent level respectively.

Appendix IIIB, Figure 5: Validity of Regression discontinuity assumptions – Density around the threshold



Notes: This suggests that criminals are able to manipulate elections. This seems to hold for the close elections with a winning margin +/- 10%.

Appendix IIIB, Figure 6: McCrary test



Notes: Density graph based on the DCdensity program code from http://eml.berkeley.edu/~jmccrary/DCdensity/. The x-axis display the margin between a criminal winner and a non-criminal runner-up in close elections with a winning margin +/-10%.

Declaration according to §16 (Assurances) Examination Regulations for the doctoral programme in Economic Sciences

1. The opportunity for the existing doctoral project was not made commercially available to me. Especially, I have not engaged any organisation that seeks thesis advisors against a fee for the preparation of dissertations or performs my obligations with respect to examination components entirely or partly.
2. I declare that I have prepared the submitted dissertation (title follows) independently and without prohibited aids; I have not accepted external help either free-of –charge or against a fee and will maintain this also in the future. I did not make use of any aids and papers other than those indicated by me. I have marked all word-by-word (direct) or implied citations of the writings by other authors.
3. I will adhere to the guidelines to ensure good scientific practice at the University of Göttingen.
4. No equivalent doctoral studies have been applied for at a different university in Germany or abroad; the dissertation submitted or parts thereof have not been used in any other doctoral project.
5. Furthermore, I am aware of the fact that untruthfulness with respect to the above declaration repeals the admission to complete the doctoral studies and/or subsequently entitle termination of the doctoral process or withdrawal of the attained title.

_____

Date, Signature

Kai Gehring Web: www.kai-gehring.net

Dr. Kai Sebastian Gehring

Date of Birth	29. April 1985
Birthplace	Karlsruhe, Germany
Nationality	German

Education

09/2011- 08/2015
Dissertation
Ph.D. in *Economics* supervised by Prof. Dr. Axel Dreher at Heidelberg University and the University of Göttingen. Graduate Program at the University of Mannheim and member of the RTG 1723 "Globalization and Development" at the University of Göttingen and Hannover.
Final grade: *Summa cum laude*

09/2014 - 12/2014
Guest researcher at the Weatherhead Center for International Affairs at Harvard University in Boston, Massachusetts

04/2013 - present
External lecturer at the University of Applied Sciences in Kaiserlautern, Germany

02/2009 - 02/2011
Graduate Degree
Diplom (M.Sc equivalent) in *Business Administration* w. electives in *Economics* at the University of Mannheim
Final Grade: *1.5 (excellent)*

02/2008 – 12/2008
Study Abroad
Studies in *Business and Economics* at the University of Canterbury in Christchurch, New Zealand

09/2005 - 06/2007
Diploma
Vordiplom (Bachelor equivalent) in *Business Administration* at the University of Mannheim, Final Grade: 1,7 (very good)

08/1995 – 06/2004
Abitur
Heinrich-von-Zügel Gymnasium, Murrhardt
Final grade: *1.5 (excellent)*

Language and IT Knowledge

Languages
English: business fluent (TOEFL 111 from 120 points)
French: Good knowledge
Spanish: Basic knowledge

IT
Application software: *Excel, Word*, and *PowerPoint*

Statistics software: *Stata, SPSS, Matlab*

Programming languages: *SAP, SQL-Databases, Visual Basic, VBA*

Kai Gehring Web: www.kai-gehring.net

Publications (peer-reviewed)

2014 Gesture politics or real commitment? Gender inequality and the allocation of aid (with Axel Dreher and Stephan Klasen), *World Development* 70: 464–480 (2015)

2013 Inequality and happiness: When perceived social mobility and economic reality do not match. (with Christian Bjørnskov,Axel Dreher, Justina .A.V. Fischer, and Jan Schnellenbach), *Journal of Economic Behavior & Organization* 91: 75 - 92 (2013)

Who benefits from economic freedom? Unraveling the effect of economic freedom on subjective well-being, *World Development* 50: 74-90 (2013)

2012 Does aid buy (economic) freedom? (with Axel Dreher), In James Gwartney, Robert Lawson, and Joshua Hall, *Economic Freedom of the World: 2012 Annual Report* (Fraser Institute): 219–246 (2012)

Discussion Papers

2015 Crime, Incentives and Political Effort: A Model and Empirical Application for India (with T. Florian Kauffeldt and Krishna Chaitanya Vadlamannati), *Courant Research Centre Discussion Papers No. 170* (2015)

2013 The home bias in sovereign ratings (with Andreas Fuchs), *University of Heidelberg Department of Economics Discussion Paper Series No. 552* (December 2013)

Information transmission within federal fiscal architectures: theory and evidence (with Axel Dreher, Christos Kotsogiannis, and Silvia Marchesi), *CESifo Working Paper No. 4400* (September 2013)

Gesture politics or real commitment? Gender inequality and the allocation of aid (with Axel Dreher and Stephan Klasen), *United Nations University - WIDER WP/2013/079* (August 2013)

Geopolitics, aid and growth (with Axel Dreher and Vera Eichenauer), *CESIfo Working Paper No. 4299* (2013)

Inequality and happiness: When perceived social mobility and economic reality do not match (with Bjørnskov, C., A. Dreher, J.A.V. Fischer, and J. Schnellenbach), *Munich Personal RePEc Archive MPRA Paper No. 44827* (2013)

Teaching

Heidelberg University

Summer term 2013 & 2014: *Introduction to Econometrics using Stata for BA/MA theses* (self-developed course)

Winter term 2011/2012: *Macroeconomics* (tutorial, with Vera Eichenauer, Alexandra Rudolph, Diego Hernandez, and Maya Schmaljohann).

Winter term 2013/2014: *Development Aid* (Bachelor Seminar)

Summer term 2013: *Economics and Politics of International Economic Organizations* (Bachelor Seminar)

Winter term 2012/2013: *Development Aid* (Bachelor Seminar)

	Winter term 2011/2012: *Macroeconomics* (tutorial, with Nicolás Corona Juárez, Andreas Fuchs and Maya Schmaljohann).
FH Kaiserslautern – University of Applied Sciences	External Lecturer since 2013: Annual course "*Introduction to Business and Economics*" within the degree program "IT-Analyst". Self-developed lecture and curriculum.
University of Mannheim	2009-2010: *Marketing Management* (Tutorial)

PhD education

9/2014-12/2014	**Specialized courses during research stay at Harvard** *Comparative Economic Development* (Nathan Nunn and James Robinson) *Political Economics* (Alberto Alesina) *Applied Econometrics* (Joshua Angrist)
9/2013 - 12/2013 **University of Mannheim**	**Specialized courses** *Econometrics of Panel Data and Social Interactions* (Stephen Kastoryano)
04/2012 - 6/2013 **University of Göttingen (w/ guest lecturers)**	**Core courses** *Micro Development Economics* (Ashok Rai) *Macro Development Economics* (Holger Strulik) *Globalization* (Axel Dreher) *Econometrics I* (Patrick Puhani) *Econometrics II* (Marcela Ibanez, Tatjana Krivobokova)
	Specialized courses *Empirical Macro Development* (Carl-Johan Daalgard) *Econometric issues in international economics* (Ron Davies) *International Economics* (Peter Egger) *Institutions and Growth* (Thorsten Beck) *Impact Evaluation* (Jenny Acker) *Poverty and Vulnerability* (Stephan Klasen)
07/2012	**Specialized courses** *The German Socio-Economic Panel (SOEP)*
09/2011 - 12/2012 **University of Mannheim**	**Core courses:** *Mathematics for Economists* (Simone Göttlich) *Advanced Microeconomics I* (Philipp Schmidt-Dengler) *Advanced Macroeconomics I* (Phillip Jung) *Advanced Econometrics I* (Andrea Weber)

Conferences and talks

2015 *European Public Choice Society Annual Meeting 2015*, April 7-10, at Groningen University, Netherlands

CSAE Conference 2015: Economic Development in Africa, March 22-24, at Oxford University, U.K.

2014 Invited talk "On Credit Rating Agencies" at the Summer School "*Country Risk Analysis*", Maastricht University

7th Workshop "*Beyond Basic Questions*", June 19-21, at Heidelberg University, Germany

Crisis, Institutions, and Banking Union Workshop, 16-17 June, Berlin, at the Federal Ministry of Finance, organized by the Hertie School of Governance

17th World Congress of the International Association, 6 - 10 June, Amman and Dead Sea, Jordan

19th Spring Meeting of Young Economists (SMYE), April 24th to 26th, at the Vienna University of Economics and Business

2014 Annual Conference of the Royal Economic Society, 7-9 April, at University of Manchester, UK

European Public Choice Society Meeting 2014, 3-6 April, at Cambridge University, UK

7th Annual Conference on The Political Economy of International Organizations, 16-18 January, at Princeton University, United States

2013 United Nations UNU-WIDER *ReCom Workshop "Aid for Gender Equality"* 16 December, in Copenhagen, Denmark

Northeastern Universities Development Consortium Conference, 3 November, at Harvard University, Massachusetts, USA

Annual Meeting of the International Political Economy Society, 24-26 October, at Claremont Graduate School, California, USA

6th Workshop "*Beyond Basic Questions*", June 13-15, 2013 at the University of Lucerne in Engelberg, Switzerland

European Public Choice Society Meeting 2013, 3 - 6 April, at ETH Zurich, Switzerland

6th Annual Conference on The Political Economy of International Organizations, Heidelberg University and University of Mannheim, 16-18 January, at University of Mannheim and Heidelberg University

2012 *Beyond Basic Questions (BBQ)* conference 2012, University of Groningen, 12-15 July, at Schiermonnikoog

Kai Gehring Web: www.kai-gehring.net

Media coverage (discussing my research)

Bilan, Börsen-Zeitung, Český rozhlas, China Daily, Der Kurier, Der Spiegel, Der Standard, Die Zeit, Financial Times (blog), Economonitor, Finanz-World, GeoMeans (blog), Финмаркет, Handelszeitung, Marginal Revolution (blog), Mint, Pravda, Punditokraterne, Slate (blog), Stickman's Corral (blog), Vårt Land, Wall Street Journal (blog), Wirtschaftswoche

Professional Experience

04/2011 - 07/2011	Intern at Simon Kucher & Partners, the worldwide leaders in Pricing consulting. Implemented and concluded 3 consulting projects in the area of banking concerning savings accounts, working capital loans, and mortgage lending.
01/2010 - 02/2010	Intern at the Center for European Economic Research ZEW: Structure and efficiency of the German electricity market; empirical analysis of retail pricing and competitive behavior.
9/2009 - 12/2010	Teaching assistant in "Marketing I“ at the Institute of Professor Dr. Dr. h.c. Homburg. Student assistant responsible for projects, lecture preparation, and empirical work/ data collection.
07/2009 - 09/2009	Internship at IBM Business Consulting: Launch, coordination and implementation of the Smarter Planet Campaign.
03/2008 - 10/2008	Student trainee at the Christchurch City Council CCC in New Zealand. Independently implemented and completed market research and consulting projects.
07/2007 - 08/2007	Internship in the department of „Coordination Sales and Marketing“ (In-house Consulting) at Bosch GmbH. Projects in sales (training concepts) and in innovation management
2/2007 - 11/2007	Student assistant at the chair of Finance Prof. W. Bühler

Further Education/Scholarships/Involvement

Student workshops	„*Science and Ethics*“, 2-day workshop with BASF AG, etc.
2008- present	*DAAD Scholarship* holder and *Alumni*: engagement in support of new scholarship holders through seminars
2/2007 -5/2007	SAP-workgroup with the *Mannheim Finance & Controlling* club
Until 2003	Participant in the “*History Competition of the German Federal President”*/ Participant in the “*Model United Nations*” Baden-Württemberg and involved in youth work for the SPD